Institutions and Identity in Economic Decision-Making

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Zusammenfassung

Die vorliegende Dissertation analysiert die Rolle von Identität und Institutionen in ökonomisch relevanten Entscheidungssituationen. Es wird argumentiert, dass ökonomisches Entscheidungsverhalten zumeist durch ein Zusammenspiel von Vorstellungen charakterisiert ist, die Menschen von sich selbst und anderen Personen haben als auch von den Normen, die diese Vorstellungen einschränken. Im Rahmen dieser Arbeit wird dieses Zusammenspiel in einer Reihe von Laborexperimenten in drei wirtschaftlichen Situationen untersucht: Eine Tauschbeziehung, eine Situation zur Arbeitnehmeranstellung, und zwei Aufgaben mit Ressourcenverteilung. Insgesamt betonen die hier dargestellten Ergebnisse die Bedeutung von Identität und Institutionen, um ökonomisches Entscheidungsverhalten besser zu verstehen. Des Weiteren zeigen die vorgestellten Befunde die Nützlichkeit von Laborexperimenten bei der Erforschung grundlegender Fragen in Bezug auf Identität und Institutionen.

Die Kapitel dieser Dissertation basieren auf den Identitätstheorien von Akerlof und Kranton (2000) und Benabou und Tirole (2011) sowie auf der Theorie der Institutionenökonomik von North (1991). Die Identitätstheorie von Akerlof und Kranton setzt soziale Verschiedenheit voraus und betrachtet, wie soziale Kategorien bestimmte Verhaltensvorschriften mit sich bringen. Die Theorie von Benabou und Tirole legt den Schwerpunkt auf moralisches Verhalten und darauf, wie Vorstellungen von sich selbst und der Umwelt die gegenwärtigen Entscheidungen eines Individuums beeinflussen. Dabei spielen Institutionen in beiden dieser Theorien eine bedeutende Rolle. Douglass North (1991, S.3) definiert Institutionen als "*humanly devised constraints that structure political, economic, and social interactions.*" Im ersten Kapitel der Dissertation werden die grundlegenden Konzepte erläutert, an denen sich die Untersuchungen der nachfolgenden Kapitel orientieren.

Die in Kapitel 2 vorgestellte Studie ist in Eigenarbeit entstanden und trägt den Titel *"To friends everything, to strangers the law: An experiment on contract enforcement and group identity."* Diese Studie orientiert sich an der Behauptung von Akerlof und Kranton, dass es identitätsbasierte Auszahlungen gibt, die sich von eigenen Handlungen und den Handlungen anderer

Personen ableiten. Darüber hinaus wird überprüft, wie eine außenstehende Partei diese Auszahlungen in Form von Vertragsdurchsetzung verändern kann. Das Ziel des Kapitels ist es, die wesentlichen Charakteristika formeller und informeller Institutionen herauszufiltern und deren Auswirkungen auf Vertrauen und Verhalten in Tauschbeziehungen aufzuklären. Formelle Institutionen wurden mittels Vertragsdurchsetzung durch Dritte abgebildet, während informelle Institutionen als gemeinsame Gruppenidentität operationalisiert wurden. Die Ergebnisse zeigen, dass Vertrauen mit zunehmender Wahrscheinlichkeit von Vertragsdurchsetzung durch Dritte ansteigt, aber nicht von gemeinsamer Gruppenidentität beeinflusst wird. Allerdings lässt sich feststellen, dass dem entgegengebrachten Vertrauen eher in Interaktionen mit Mitgliedern der Eigengruppe als der Fremdgruppe nach-gekommen wird.

Kapitel 3 entstand in Zusammenarbeit mit Alexia Gaudeul und Ayu Okvitawanli und befasst sich mit der Fragestellung *"Does the gender mix among employers influence who gets hired? A labor market experiment."* In diesem Kapitel wird die Theorie von Akerlof und Kranton erneut überprüft, wobei das Identität hier Geschlecht betrifft. Es wird der Frage nachgegangen, ob die geschlechtliche Zusammensetzung auf Ebene der Entscheidungsträger in Unternehmen Aus-wirkungen auf die Geschlechterverteilung auf Mitarbeiterebene hat. Im Rahmen eines Laborexperiments wurden einer Gruppe bestehend aus zwei unabhängigen Arbeitgebern Bewerbungen von zwei potentiellen Mitarbeitern präsentiert. Dabei wurde untersucht, ob das Geschlecht des jeweils anderen Arbeitgebers der Gruppe die Entscheidung beeinflusst, welcher Mitarbeiter eingestellt wird. Die Ergebnisse veranschaulichen, dass weibliche Arbeitgeber mit höherer Wahrscheinlichkeit einen weiblichen Bewerber anstatt eines gleich geeigneten männlichen Bewerbers auswählen, wenn sie mit männlichen Arbeitgebern in einer Gruppe sind. Die Ergebnisse eines impliziten Assoziationstests (IAT) sowie die Antworten auf einen post-experimentellen Fragebogen zeigen, dass explizite Ansichten hinsichtlich relativer geschlechtsspezifischer Leistung signifikant mit den beobachteten Neigungen beim Einstellungsverhalten zusammenhängen. Implizite Ansichten schei-nen hingegen keine Rolle zu spielen.

Kapitel 4 trägt den Titel *"On the malleability of fairness ideals: Order effects in partial and impartial allocation tasks."* Es entstand in Zusammenarbeit mit Kathrin Dengler-Roscher, Natalia Montinari, Matteo Ploner, und Benedikt Werner. Die Arbeit zu diesem Kapitel ist geprägt von Benabou und Tiroles Theorie zu moralischer Identität. Anhand eines Laborexperiments wird erforscht, ob sich die Gerechtigkeitsvorstellungen der Teilnehmer unterschei-

den, je nachdem, in welcher Reihenfolge sie zwei Verteilungsaufgaben bearbeiten. Zunächst erwirtschaften die Teilnehmer durch die Bearbeitung einer *"real effort task"* Ressourcen. Diese werden anschließend in zwei Aufgaben aufgeteilt verteilt. In der Verteilungsaufgabe mit eigener Beteiligung *(partial allocation task)* bestimmt ein Teilnehmer den eigenen Verdienst und den Verdienst eines anderen Teilnehmers. In der Verteilungsaufgabe ohne eigene Beteiligung *(impartial allocation task)* bestimmt ein Teilnehmer über den Verdienst zweier anderer Teilnehmer. Zudem wurde der Aspekt der Vorerfahrung der Teilnehmer variiert, d.h. ob diese bereits an einem ähnlichen Verteilungsexperiment teilgenommen haben. Die Ergebnisse zeigen, dass es unwahrscheinlicher ist, dass sich Teilnehmer bei eigener Beteiligung selbst mehr Ressourcen zuzuteilen als sie in der *"real effort task"* erwirtschaftet haben, wenn sie zuerst ihre Entscheidung ohne eigene Beteiligung getroffen haben. Dieser Effekt konnte jedoch nur unter Teilnehmern festgestellt werden, die nicht an vorigen Experimenten teilgenommen haben.

Im abschließenden Kapitel 5 werden die zentralen Ergebnisse der Experimente diskutiert. Zudem werden sich daraus ableitende Implikationen erörtert als auch Anschlussmöglichkeiten für zukünftige Forschungen aufgezeigt.

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To be part of a doctoral program called the *International Max Planck Research School (IMPRS) on Adapting Behavior in a Fundamentally Uncertain World* is to grapple with uncertainty not just as a subject of investigation but also as an inevitable part of research. Although this dissertation bears my name, there were many people who helped me deal with the uncertainty of bringing this into fruition.

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[1] Paraphrased from a dialogue in Veronica Mars, Season 2, Happy-Go-Lucky (2006).

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1. Introduction

> *Who in the world am I? Ah, that's the great puzzle!*
> *(Alice in Lewis Caroll's Alice's Adventures in Wonderland, 1865, p. 18)*

From Thales the Greek philosopher to Alice the popular child character, the question of "Who am I?" has plagued many human beings since time immemorial. Though the path to self-knowledge may be difficult, an idea of one's self is nevertheless essential in economic decision-making. As Akerlof and Kranton (2010, p.4) write, "[i]n every social context, people somehow have a notion of who they are which is associated with beliefs about who they and other are supposed to behave." Indeed, if one looks at the economic decisions people make, people's notion of who they are play a crucial role—in the choice of who to do business with, in the choice of who to hire, or in the choice of how much to compensate yourself and others. These situations invite an identity-based analysis.

The standard theory of a rational economic man fails to consider that most economic choices are made in a social setting wherein one's notion of self play a role. In the standard theory of a rational economic man, the choices simply reflect basic tastes or preferences. But as Akerlof and Kranton (2010, p.4) point out, "these are not basic tastes such as 'I like bananas' and 'You like oranges.'"[1] These tastes can come from rules regarding how people should behave in different situations and these rules can be tied with a person's notion of self. Rules can also be external to one's self, or as what Douglass North (1991) refers to as institutions, or the humanly devised constraints that shape behavior.

In this dissertation, I argue that an identity- and institutions-based analysis of economic interactions can bear fruitful insights. Although there is by now a significant body of work that investigates the role of identity and institutions in economic decisions, most of these analyses occur at a level beyond the individual, e.g., at the firm- or country-level. Moreover, causal

[1] Even in these cases, some knowledge of one's own preferences is manifested.

links remain difficult to pin down. This dissertation aims to help address the gap by analyzing identity and institutions at the level of individual choice and behavior.

More precisely, I attempt to test the behavioral assumptions and predictions of two theories of identity in economics. The first theory is by Akerlof and Kranton (2000) that models identity that is premised on social difference while the second theory is by Bénabou and Tirole (2011) that models identity that arises from a process of self-inference. I test these theories in a series of laboratory experiments, with each experiment focusing on some form of identity and how it affects behavior in a specific economic setting. In the first study, I investigate how knowledge of shared group membership affects outcomes in an exchange relation; in the second study, I focus on how the gender mix of employers affects hiring decisions; and in the third study I analyze how fairness ideals change in consecutive allocation decisions.

I present in the following chapters of this dissertation experiments that are illustrative of more complex economic interactions wherein notions of identity play a role. They are not meant to be adequately representative of the whole spectrum of identity-related behavior. Rather, they were designed to distill the effect of a type of identity on behavior and to root certain economic phenomena observed at a broader level at the level of individual choices. For these reasons, I use laboratory experiments as my main tool of investigation and draw insights from economics, psychology, political science, sociology, and law to interpret my results.

This chapter is organized as follows. Sections 2 and 3 elaborate on economic models of behavior that incorporate identity. In both these sections, I also briefly explain the possible role institutions play in shaping identity and consequently, behavior. Section 4 discusses the use of laboratory experiments in exploring questions on identity and institutions. The final section gives an overview of the subsequent chapters in the dissertation and presents how each study was conceived and executed.

1.1. A model of identity by Akerlof and Kranton

> *And she began thinking over all the children she knew that were of the same age as herself, to see if she could have been changed for any of them. 'I'm sure I'm not Ada,' she said, 'for her hair goes in such long ringlets, and mine doesn't go in ringlets at all; and I'm sure I can't be Mabel, for I know all sorts of things, and she, oh! she knows such a*

> *very little! Besides, SHE'S she, and I'm I, and—oh dear, how puzzling it all is!*
>
> *(Alice in Lewis Caroll's Alice's Adventures in Wonderland, 1865, p. 19)*

Adam Smith (1759) uses the metaphor of a mirror in *The Theory of Moral Sentiments* to illustrate how we learn to define and judge our selves in relation to other people.[2] In a thought experiment, he asks the reader to imagine a human who grew up in a solitary place, without contact with other humans. Such a human being could no more think of the priopriety of his conduct as the beauty of his own face. Only upon being introduced to other humans, is he "immediately provided with the mirror which he wanted before." In the words of Vernon Smith (2014), Adam Smith makes the point particularly in this passage and more generally in The Theory of Moral Sentiments, "there is no self unless there is a we."

Akerlof and Kranton's model of identity is thus premised on social difference. They formally introduced the concept of identity in economic analysis as based on social categories. Each person has a mapping of her own category and other people's categories. Each category has a set of prescriptions that indicate what the appropriate behavior is for persons belonging to that category.

A person's identity depends on her assigned social category, to what extent her own given characteristics matches the ideals of her assigned category, and to what extent her actions and other's actions follow their respective categories' prescriptions.

A person's utility thus depends on a their actions, the actions of others, and on their identity. In the simplest case, an individual chooses actions to maximize utility, taking the category, their respective category, and prescription as given. In some cases, the category assignment can also be chosen.

The key results of Akerlof and Kranton's analysis of identity related behavior that inform the work in Chapters 2 and 3 are as follows: (1) people have identity-based payoffs derived from their own actions; (2) people have identity-based payoffs derived from others' actions; and (3) third parties can generate (persistent) changes in these payoffs.

In Chapter 2, I test whether knowledge of shared group membership affeccts the payoffs of two parties in an exchange relationship while in Chapter 3, I investigate whether an employer is more likely to hire an applicant of

[2] See Part III.Of the Foundation of our Judgments concerning our own Sentiments and Conduct, and of the Sense of Duty, Chapter I.3.

the same gender, holding everything else equal. The third party in Chapter 2 comes in the form of an exogenous chance move to represent the quality of (formal) institutions. Meanwhile, I manipulate in Chapter 3 the gender mix of employers to capture how an exogenous policy like a gender quota can affect individual hiring decisions.

Akerlof and Kranton's theory of identity in economics follows a long line of research on group identity and discrimination in social psychology. Tajfel and Turner (1986) pioneered a social identity theory that posits that people discriminate to maintain a positive social identity with their in-group. Yamagishi et al. (1998) provides an alternative theory that rests on bounded group reciprocity. Favoring an in-group over an out-group member is the result of an evolved decision heuristic that leads individuals to strive for a reputation as a cooperator, thus securing future indirect benefits and reducing the probability of being excluded from the group.

These theories on group identity and discrimination underlie the work in Chapters 2 and 3. For example, Akerlof and Kranton's theory provides a microfoundation for earlier models for discrimination models by Becker (1971), Arrow (1973), and Aigner and Cain (1977) which are discussed in Chapter 3. It also serves as a basis for using the method by Tajfel et al. (1971) to establish the minimal conditions for discrimination to arise in Chapter 2.

1.2. A model of identity by Bénabou and Tirole

> *"I could tell you my adventures—beginning from this morning," said Alice a little timidly; "but it's no use going back to yesterday, because I was a different person then."*
>
> *(Lewis Caroll's Alice's Adventures in Wonderland, 1865, p. 91)*

Walt Whitman (1860) captures a central puzzle of moral behavior in the following lines of his poem, *A Song of Myself*: "Do I contradict myself? Very well then I contradict myself, (I am large, I contain multitudes)." Indeed, the apparent ease with which people contradict themselves also puzzled Bénabou and Tirole (2011, p.806) who ask why people who previously manifested altruistic behavior revert so easily to selfishness with the "most transparent change in framing" and "with the thinnest of veils." To solve this puzzle, Bénabou and Tirole (2011) develop a theory of moral behavior that is premised on a model of identity whereby people infer their own values

from past choices. I discuss in this section key points that inform the work that is presented in Chapter 4.

If the self in Akerlof and Kranton's model arises from one's beliefs defined in relation to others, the self in Benabou and Tirole is borne through beliefs about one's values. In Benabou and Tirole's theory, beliefs function like assets, although people do not have an objective access to them. People judge themselves by what they have done because they have better access to a record of what they have done than to the beliefs that underlie them. Thus, when deciding what to do in the present, people consider what kind of a person each alternative would "make them" and the desirability of those self-views.[3] If social reputation is emphasized in Akerlof and Kranton's theory, self-reputation is highlighted in Benabou and Tirole's theory.

A self-reputational perspective helps clarify many puzzles of moral behavior according to Benabou and Tirole, particularly on why moral behavior changes so easily with the slightest and most transparent change of framing. Because of imperfect self-knowledge, people are more likely to be affected by minor manipulations of salience especially when objective information about deep preferences such as true generosity, loyalty or faith is scarce. A self-reputational perspective also implies history-dependence in behavior as one forms a reputation on the basis of previous actions.

In Benabou and Tirole's framework, institutions can affect moral behavior because people decide what to do in the present not only according to information from their past actions but also cues from their present environment. Institutions can make more apparent messages to encourage certain behavior.

Chapter 4 tests the implications of history-dependence in decisions concerning fairness. In particular, I test whether the sequence in which people decide in two types of allocation tasks change their decisions about fairness. Applying Benabou and Tirole's framework, I assume that objective information about a person's preferences for fairness is scarce. Thus, decisions concerning fairness would be quite malleable. To test for history-dependence and to make salient different messages, I manipulate the order in which participants perform the two decision tasks. In one treatment, I induce participants to behave prosocially by deciding impartially; in another, I induce participants to behave selfishly by deciding partially. Within this

[3] Benabou and Tirole mention that their theory is decidedly cognitive because of this self-inference process. Dual process models and the idea of multiple selves in economics as summarized in Alos-Ferrer and Strack (2014) also fit in this framework.

framework, participants' choices in the subsequent task can be affected in two ways: the action can amplify the original manipulation or it can be in opposition to it. Previous research in psychology also bear these predictions out. To illustrate how previous manipulations can magnify subsequent actions, DeJong (1979) has documented the "foot-in-the-door" effect which shows how an initial small request accepted raises the probability of accepting costlier ones later on. In contrast, participants can display moral credentialing or balancing, acting as if an initial good behavior becomes a license to misbehave later on as Monin and Miller (2001) show.

1.3. The use of experiments

> *"The first thing I've got to do,' said Alice to herself, as she wandered about in the wood, "is to grow to my right size again; and the second thing is to find my way into that lovely garden. I think that will be the best plan."*
>
> *It sounded an excellent plan, no doubt, and very neatly and simply arranged...*
>
> *(Lewis Caroll's Alice's Adventures in Wonderland, 1865, p. 37)*

Given that the aim of the dissertation is to test behavioral assumptions and predictions of theories of identity, [4] I chose laboratory experiments as my main method of investigation. There are several features of this method that serve this dissertation's purpose. First, it gives me sufficient control over which factors I manipulate and which factors I hold constant. In contrast to field experiments, it is easier to replicate the conditions under which different participants make their decisions. This degree of control simplifies the process of establishing links between the dependent variable (i.e. economic behavior), with the explanatory variable (e.g. institutional quality, information about identity). Second, it allows me to randomize assignment of participants in a cost-effective manner which also simplifies the process of statistical inference. Third, the use of experiments to test identity-related behavior follows from psychology. Given that earlier theories and applied work on identity have come from psychology and have been later incorporated in economic models of behavior, I use experiments not only to relate

[4] Falk and Heckman (2009) cite this as one of the primary reasons why laboratory experiments as tool of investigation.

my work to the existing body of literature but also to strengthen the link between these two disciplines in studying behavior.

In the next three chapters, I go into detail why a laboratory experiment is particularly suited to answer the research questions posed. Each study presented here use laboratory experiments with student participants from Jena, Germany. I tackle key limitations of using experiments in the final chapter of the dissertation.

1.4. Overview

> *"Would you tell me, please, which way I ought to go from here?" "That depends a good deal on where you want to get to," said the Cat. "I don't much care where—" said Alice. "Then it doesn't matter which way you go," said the Cat. "—so long as I get SOMEWHERE," Alice added as an explanation.*
>
> *(Lewis Caroll's Alice's Adventures in Wonderland, 1865, p. 56)*

In this section, I give a more detailed overview of what each chapter contains and how they were conceived. I also lay-out a roadmap of how the dissertation proceeds. Each chapter can be read on its own or sequentially—as part of a progression of studies exploring identity and institutions in economic behavior.

Chapter 2 is a single-authored work entitled *"To friends everything, to strangers the law? An experiment on contract enforcement and group identity."* This chapter is inspired by the analysis of Adam Smith(1776) in *"An Inquiry into the Nature and Causes of the Wealth of Nations"* on how exchange relations within kinship ties evolved into anonymous, impersonal exchange. This is a phenomenon that is by now well-explored in cross-country studies as well as historical accounts, but analysis at the level of individual decisions remains limited. This chapter aims to contribute to this discussion. Chapter 2 distills and disentangles the relative impact of formal institutions, i.e., exogenous contract enforcement, and informal institutions, i.e., shared group identity on behavior in exchange relations. Results show that trust in an exchange relation increase as the level of contract enforcement increases but is not affected by shared group identity. However, compliance with given trust is more likely to occur in interactions with in-group members than out-group members.

After investigating the impact of nominal identities on behavior in the laboratory, I explore with Alexia Gaudeul and Ayu Okvitawanli how an identity formed outside the laboratory such as gender influences hiring decisions in a labor market experiment. Chapter 3, entitled *"Does the gender mix of employer influences who gets hired? A labor market experiment,"* is borne by my search for ways to measure discrimination in the laboratory. One of the methods used in psychology is the Implicit Association Test (IAT) which tests for implicit bias or overlearned associations between categories and specific words. Together with Ayu Okvitawanli, a fellow doctoral student in psychology, and we design and implement an experiment that tests how the gender composition of a hiring committee affects who its individual members are likely to select from a pool of applicants who are equally performing but of different genders. After running the experiment, I analyze the data with Alexia Gaudeul and we find that the gender mix among employers plays a role in the individual hiring decisions of female members. Female employers when paired with a male employer are more likely to choose a female applicant over an equally competent male applicant. Results of the Implicit Association Test (IAT) and answers to a post-experimental questionnaire show that explicit beliefs about relative gender performance are significantly associated with the observed hiring bias, while implicit attitudes do not appear to play a role.

If the previous studies explore how people's identities affect their interactions with others, Chapter 4 focuses on how people manage their identities internally. Chapter 4, "*On the malleability of fairness ideals: order effects in partial and impartial allocation tasks* " is conceived from a research proposal I presented in the first week of the IMPRS Summer School 2012. Together with fellow PhD students, Benedikt Werner and Kathrin Dengler-Roscher, and our supervisors, Natalia Montinari and Matteo Ploner, we design and implement an experiment that looks at how people form and manage their identities by inferring their personal values from past choices. We test this hypothesis through a laboratory experiment observing participants' sequential allocation decisions in a dictator game. In one treatment, participants first decide partially, i.e., they divide a pie between themselves and another subject. They then decide impartially by dividing a pie between two other subjects apart from themselves. In another treatment, the sequence of decision tasks is reversed. After conducting the experiments, I, Natalia Montinari, and Matteo Ploner analyze the data and find that participants allocate more to themselves than what they have earned when they decide partially. The sequence of roles as decision maker in the dictator

game has an effect for participants who have not yet played the dictator game-like experiments before. However, for participants who have played allocation experiments before,the sequence of allocation tasks has no effect on their decisions.

Chapter 5 critically reviews the previous chapters, discusses the implications of the results, and suggests pathways for future research.

2. To friends everything, to strangers the law? An experiment on contract enforcement and group identity[1]

2.1. Introduction

Economists have long recognized the importance of institutions in promoting and sustaining economic growth. The path-breaking work by North and Thomas (1973) lays the theoretical and empirical foundation for establishing the causal link between economic growth and institutions. North (1991, p.3) defines institutions as the "humanly devised constraints that structure political, economic, and social interactions."[2] Institutions can be formal rules or informal constraints. Acemoglu et al. (2005), Dollar and Kraay (2003), and Rodrik et al. (2004) emphasize the primary role of institutional quality, [3] particularly of formal institutions like property rights protection and rule of law, in explaining differences in economic growth between countries.

Underlying the broad function of institutions in sustaining economic growth is its specific role in supporting exchange relations. The importance of informal constraints is emphasized in this strand of literature. Greif (1993,

[1]The title comes from a Latin American quip "A los amigos todo, a los enemigos nada, al extrano la ley" in Rose-Ackerman (1999, p.97).

[2]Hodgson (2006) criticizes this definition as not sufficiently clear. This is something that I tackle in Section 2.

[3]This is measured by an index by Kaufmann et al. (2002) which takes available subjective measures of rule of law and protection of property rights, and combine them into a composite indicator.

p.525) writes that "without the ability to exchange, the potential for growth is rather limited," yet inherent in most exchange relations is a commitment problem. An agent can promise to deliver some goods upon receiving money from another agent. The first agent can act opportunistically by embezzling the money and not delivering the goods he promised. In his paper examining 11th century Maghiribi trade, Greif (1993) examines how the Maghiribis overcame the commitment problem amidst a weak legal system by forming a trading coalition based on shared ethnicity. Landa (1981) makes a similar case for ethnically homogenous middlemen groups functioning as an alternative to contractual law in early trade among the Chinese in Southeast Asia. More recent work by Rauch and Trindade (2002) show that ethnic Chinese networks, proxied by the product of ethnic Chinese population shares, increase bilateral trade for products whose quality is difficult to ascertain, in addition to enforcing community sanctions and deterring opportunistic behavior. Gould (1994) finds similar results suggesting that U.S. immigrants have historically been important in increasing bilateral trade flows with immigrants' home countries. In these examples, when legal systems were weak, individuals turned to informal institutions like ethnic networks to support trade.

While significant advances have been made in causally linking institutions with economic growth and with the evolution of exchange relations, explaining and differentiating how informal and formal rules affect individual behavior remain a challenge. Part of the difficulty lies in the fact that most behavior is embedded in an environment of both formal and informal rules which can be difficult to disentangle in empirical work. For example, there is the risk of overestimating the role of formal institutions, which are also shaped by informal constraints.[4] In some instances, formal and informal rules appear to function similarly although they can lead to divergent outcomes.[5] Understanding where such differences lie can help how we design and sequence institutional reform.[6]

This study attempts to disentangle the effects of formal and informal in-

[4]See the discussion on social capital by Knack and Keefer (1997)

[5]Helmke and Levitsky (2004) introduce a typology of formal and informal institutions: first on the basis of the degree to which formal and informal institutional outcomes converge, and second on the basis of the extent to which rules and procedures that exist are enforced and complied with in practice.

[6]The importance of sequence matters for instance, when reforms crowd-out desired behavior already existing and shaped under old rules. As Stiglitz (2000, p.66) writes, "how we sequence reforms can matter, and matter a great deal."

stitutions on behavior in exchange relations through a laboratory experiment. My approach follows the framework of Helmke and Levitsky (2004) to distill the essential characteristics of formal and informal institutions and to distinguish their relative impact on behavior in a one-shot interaction. The exchange relation depicted in the experiment involves two players who move sequentially. The first player can either enter or not enter into an agreement with the second player. For example, the first player offers money to the second player for goods to be delivered in a future date. If the first player does not offer an agreement, then no exchange takes place. If the second player performs his end of the deal, joint surplus is produced; if the second player breaches and acts opportunistically (e.g. by embezzling the money), with a given probability of contract enforcement, he can be found liable. I manipulate the quality of formal institutions through exogenous changes in the probability of contract enforcement. Meanwhile, informal institutions are created via shared expectations induced through shared group identity.

Overall, the results of the experiment show that formal institutions in the form of third party enforcement encourage cooperative behavior of both parties in an exchange relation. Shared group identity however only affects the performance of the second mover in an exchange relation.

This study is part of a growing theoretical and experimental literature analyzing institutions in exchange relations. In this respect, this work is closely related to Gueth and Ockenfels. (2003) and to Gueth and Ockenfels (2000) that present theoretical models that analyze the evolution of trust in the presence of both formal (e.g. legal insurance, courts) and informal institutions (e.g.communities). However, this paper implements an experiment to analyze formal and informal institutions in a one-shot game instead of repeated interactions. It also follows the work of Bohnet et al. (2010) who find in their cross-country laboratory experiments differences in trust and trustworthiness according to the predominance of rules-based interactions in the West and relation-based interactions in Gulf countries. I also explore these differences but in a minimal setting where groups are induced in the laboratory instead of determined by geography and ethnicity. Like Buchan et al. (2002), I analyze in the laboratory the role of social identity in one-shot anonymous interactions. In addition to their work, I consider the role of contract enforcement. In terms of analyzing exchange relations in the laboratory, my work builds upon the experiment of Kollock (1994) but instead of looking at information asymmetries as a source of differences in trust and trustworthiness, I focus on formal and informal rules.

In the next section, I outline the framework and model that I use in designing the experiment and describe how the experiment was conducted. In Section 3, I discuss the hypotheses that I test while I explain results in Section 4. I conclude and discuss avenues for future research in Section 5.

2.2. Method

In this section, I begin by presenting a framework whereby I can investigate formal and informal institutions in the laboratory. I then present the details of the game I used to model exchange relations and the experimental treatments I implemented. The final section describes procedural details of the experiment.

2.2.1. Framework

Helmke and Levitsky (2004) point out that most empirical and theoretical work on institutions have not been clear and consistent in distinguishing between what formal and informal institutions are.[7] To provide conceptual clarity, they define "informal institutions as socially shared rules, usually unwritten, that are created, communicated, and enforced outside of officially sanctioned channels" while formal institutions are "rules and procedures that are created, communicated, and enforced through channels widely accepted as official" (Helmke and Levitsky, 2004, p.727). They also note four important distinctions when discussing informal institutions to avoid casting it as a residual category of formal institutions: 1) informal institutions must be distinguished from weak institutions; 2) they are different from informal behavioral regularities;[8] 3) they must not be confused with informal actors and organizations; 4) and they must be more narrowly defined than culture—informal institutions should be defined in terms of shared expectations rather than shared values.

[7] Helmke and Levitsky (2004) also summarize previous work on formal and informal institutions as being distinguished along the lines of cultural traditions, state or civil society, or third-party or self-enforcement. They argue though that these distinctions are either too ambiguous or fail to capture crucial features.

[8] They cite here the example of removing one's hat in church as an informal institution while removing one's coat in a restaurant is simply a behavioral regularity. Leaving one's hat in the church can trigger social disapproval or sanctions while leaving one's coat does not.

Within this framework, analyzing the the effect of informal and formal institutions on behavior through laboratory experiments becomes more meaningful. The laboratory, apart from being an ideal venue to investigate individual behavior under specific conditions, allows the simulation of an environment where I can clearly and feasibly draw the distinctions just enumerated. To create a type of informal institutions in the laboratory, I systematically induce social identity in the laboratory following the minimal groups paradigm by Tajfel et al. (1971). In a typical minimal group experiment, participants are randomly and anonymously divided into two groups on the basis of trivial criteria, e.g. preference for a painting.[9]

How are nominally shared group identities like informal institutions? Yamagishi et al. (1998) suggest that differences in behavior toward an in-group member versus an out-group member in a minimal groups paradigm arise not because of shared social categories *per se* but because of shared expectations between same group members. Such shared expectations can be induced through common knowledge of shared group membership as seen in the experimental results of Jin and Yamagishi (1997). In the words of Yamagishi and Kiyonari (2000), the group becomes "a container of generalized reciprocity." As already mentioned in the work of Greif (1993), Landa (1995) and in experiments summarized in Andreoni (2005), the expectation to reciprocate functions like an informal constraint and can be a substitute for law when compliance with contracts is imperfectly enforced.

To translate in the laboratory the essential feature of shared expectations in informal institutions, I create minimal social groups and also implement common knowledge of group membership in treatments wherein group membership is revealed. As Platow et al. (2012) explain, common knowledge emerges when one person (Player 1) knows the group membership of the other person (Player 2), and knows that this Player 2 also knows the group membership of Player 1. I further underline the contrast between formal and informal rules of the game by how they are communicated to participants: the probability of contract enforcement and its possible impact on pay-offs are explicitly and directly relayed to participants to mimic an official communication. While group membership of the partner is also directly communicated to participants, whether it should have consequences on pay-offs is entirely up to the participants' actions.

Using the minimal groups paradigm helps me distinguish informal insti-

[9] See Guala et al. (2012) for a summary of the common methods of inducing minimal social groups.

tutions (i.e. group identity) from weak institutions (i.e. weak contract enforcement). Furthermore, because groups are artificially induced in the laboratory and participants play only one round, I am able to separate informal institutions created through shared expectations from behavioral regularities and common values that usually emerge from shared history. Moreover, there is no inherent organizational feature within a minimal group that determines who decides and enforces what the rules are. Thus, I am able to separate the effect of informal rules from the actors that enforce them.[10]

Participants then play the contract game by Bohnet et al. (2001) for one round. The contract game proceeds as follows. The first mover decides whether she wants to enter a contract without knowing whether the second mover will perform. Not entering the contract leaves both players with nothing while a contract offer by the first player and a corresponding performance by the second player generates joint surplus. If the second player does not perform, a chance move determines whether or not he is punished for breaching the contract. As in Bohnet et al. (2001), this chance move represents a formal institution that determines the probability of contract enforcement. In the experiment, I vary the probabilities of contract enforcement in which players make a decision and implement three group conditions: in one condition, players have no knowledge of group identity of the other party; in another condition, players have common knowledge that their partner is an in-group member; and in the last condition, players have common knowledge that their partner is an out-group member.

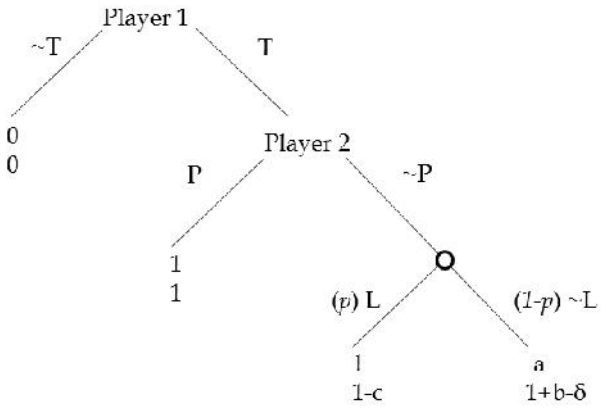
2.2.2. The contract game

The contract game[11] by Bohnet et al. (2001), henceforth BFH, models an asymmetrical relationship between two parties who can enter a contract to produce a surplus. The relationship is asymmetrical in the sense that Player 1 first enters the contract without knowing whether Player 2 will perform.

[10] On a related note, I also accomplish something similar by devising contract enforcement as an exogenous move and not as a strategic choice by another player. This allows me to remove the impact of formal actors who enforce the rules (e.g. police officers or judges) and retain the effect of the rules of the game (e.g. when someone is found liable) per North (1991).

[11] I follow the terms here used by BFH for consistency with their work. For simplicity, contract here can be thought of as a legally enforceable agreement that an exchange will take place.

As BFH note, Player 1's decision to enter a contract is a matter of trust.[12] I summarize here the essential features of the game while I presents the game in extensive form in Figure 2.1.



Note: Player 1 can either offer a contract or not offer a contract to player 2. If she does not trust, she and player 2 get nothing. If she trusts, player 2 decides whether to perform or not perform the contract. If he performs, both he and player 1 get 1. If he does not perform, a chance move determines whether he is found liable. If he is found liable, player 1 gets 1 and player 2 suffers a cost from breaching and gets $1 - c$. If he is not found liable, player 1 suffers the costs of breach and gets $-a$ and player 2 gets $1 + b - \delta$.

Figure 2.1.: The contract game (Bohnet et al., 2001)

The contract game is a sequential, two-person game. The first player (Player 1) has two options, to either trust (T) or not trust ($\tilde{T}$). If she does not trust, both she and Player 2 get *0*. If she chooses to trust, Player 2 then gets to decide between performing (P) or not performing ($\tilde{P}$). If he performs, both he and Player 1 get 1.[13] If Player 2 breaches the contract and does not perform, then a chance move that captures a litigation process comes into effect.[14] With a probability p, Player 2 can be found liable (L) and he bears

[12] Given that the game is formulated such that an exogenous probability influences the decision of Player 2 and consequently, the decision of Player 1, it may be more precise to describe this as calculative trust following the typology of Williamson (1993) and label the actions available to Player 1 as 'offer' or 'not to offer' a contract.

[13] I use female pronouns for Player 1 and male pronouns for Player 2 for purposes of distinction only.

[14] This is similar to Abbink et al. (2002) modeling discovery of committing bribery as an exogenous lottery in their experimental bribery game and to the seminal work of Becker (1968) which takes into account the probability of punishment in the commitment of crimes. I adopt this simplification in line with the study's research question. One can

the costs of the trial *c*; Player 1 gets *1* and Player 2 gets $1 - c$.[15] If he is not found liable, Player 1 gets - *a* and Player 2 profits from the breach and receives $1 + b - \delta$, where $b > 0$. However, the gains from breaching are not large enough to compensate the first mover, that is $b < 1 + a$. The parameter δ represents the psychological costs for breaching a contract.[16]

A higher *p* indicates a stronger institutional or in this case, contract enforcement, environment. That is, it is more likely for the chance move to find Player 2 liable if he did not perform the contract; a lower *p* signifies that it is less likely that Player 2 is found liable if he breaches.

2.2.3. Treatments

I manipulate two variables in the experiment: group identity to represent informal institutions and level of contract enforcement to capture formal rules. Within subjects, I manipulate the probability of contract enforcement *p* like in the experiment of BFH. Between subjects, there are 3 group identity conditions: no-knowledge, in-group, and out-group. In the no-knowledge condition, players are not informed of the identity of the other player in the contract game; in the in-group condition, both players are informed that the other player in the contract game comes from the same group; and in the out-group condition, both players are informed that the other player in the contract games comes from a different group. Moreover, in the in-group and out-group condition, both players know that their partners know their respective identity and their partner's identities. Table 2.1 shows how many participants were assigned the role of Player 1 and 2, and the group conditions they were assigned.

of course explicitly model authorities as strategic players as in Basu et al. (1992) or as rational belief-forming institutions like in Gueth and Ockenfels (2000) and Brennan et al. (2003).

[15] The configuration of payoffs assumes perfect expectation damages which put Player 1 in a position as if Player 2 had performed the contract and that Player 2 shoulders all costs of the trial.

[16] This is similar to *m* or the moral costs of breaching as in Gueth and Ockenfels. (2003).

Table 2.1.: Number of participants and player types in each treatment

	No group	In-group	Out-group	Total
Player 1	30	30	30	90
Player 2	30	30	30	90
Total	60	60	60	180

In implementing the group conditions, I consider the proposition of Akerlof and Kranton (2000) that there are identity-based pay-offs and the psychological costs of breaching for in-group interactions are higher than out-group interactions.[17] More formally, this implies that $\delta_I > \delta_O$ where I represents an interaction with an in-group member and O represents an interaction with an out-group member.

2.2.4. Procedures and payoffs

Participants in all treatments and upon entering the laboratory are arbitrarily assigned to a group—-red or yellow—and a role—Player 1 or Player 2. Similar to how Guala et al. (2012) induced groups in one of their treatments, participants in this experiment drew from a covered box at the entrance of the laboratory either a yellow or red arm band which indicated their group identity and their computer number that also determined whether they are Player 1 or Player 2.[18] Each participant was assigned a visually isolated computer terminal that corresponded to the number indicated on their arm band. The group identity and role of each participant remained the same throughout the experiment and there was an equal probability of drawing a red arm band and a yellow arm band and of being assigned the role of Player 1 and Player 2.

All participants received at the beginning of the experiment a copy of the instructions. The instructions contained some passages from the instructions of Chen and Chen (2011) to relay group information and some passages from BFH to explain the mechanics of the contract game. The instructions which are presented in Appendix A.1 were neutrally framed to minimize presentation or context effects that are not related to group iden-

[17] Balliet et al. (2015) conducts a meta-analysis of the experimental evidence on this phenomenon.

[18] The program allocated half of the computers in the laboratory for Player 1 and the other half for Player 2.

tity or contract enforcement probabilities; in particular, choices were labeled with letters instead of trust or not trust and perform or not perform. Each participant received a copy of the instructions for both Players 1 and 2 and an experimenter also read aloud the instructions. This is to ensure that all participants have common knowledge of how the experiment proceeds regardless of player type. Before participants made decisions, participants answered questions to test their understanding of the instructions and they could only proceed with the experiment upon answering all control questions correctly.

Participants made decisions via strategy method, i.e. they made contingent decisions for all the probabilities at which they may have to play.[19] Player 1 decided to either trust or not trust Player 2 under changing probabilities of contract enforcement. The probabilities of contract enforcement changed by increments of 0.10, beginning from $p = 0.1$ until $p = 0.9$. Player 2 chose to perform or not perform under different probability conditions based on the hypothetical situation that Player 1 trusted them. All decisions were payoff relevant: at the end of the experiment, a probability condition was randomly chosen and a participant was paid according to his or her decision under this probability condition. I calibrated the parameters of the model and translated them to material payoffs measured in experimental currency units (ECUs) following BFH.[20] Thus, $a = 20$,$b = 100$, and $c = 30$. This was shown to participants in the decision tree as seen in Figures A.1 and A.2 in Appendix A.1.

After playing the contract game, participants answered a post-experimental survey with items to measure their general trust from Yamagishi and Yamagishi (1994) and from the World Values Survey (2014). The items required participants to rate statements by choosing one of the following options: strongly agree, agree, neither agree or disagree, disagree, strongly disagree.

I ran the experiment with z-Tree (Fischbacher, 2007) and recruited participants through ORSEE (Greiner, 2004). The experiment's participants were students from the Friedrich Schiller University and University of Applied

[19] Such a method of eliciting participants' decision may be too psychologically cold as to capture real world behavior as Brandts and Charness (2011) point out in their survey of laboratory experiments comparing the direct response and strategy method. However, the strategy method provides a larger number of observations while still retaining the one-shot characteristic of the game and with a smaller number of participants to be paid. The survey study also finds in no case that a treatment effect found with the strategy method that is not observed with the direct response method.

[20] In the experiment 1 ECU = 0.05 Euros.

Sciences in Jena and were paid 2.50 Euros for showing-up on time. The average payment was 7.57 Euros. The sessions took place at the Goethe Galerie Laboratory in Jena on December 2013 and March 2014. The mean age of participants was 23.58 years, 62.22 percent were females, and 94 percent wrote Germans as their nationality.

2.3. Hypotheses

The first hypothesis takes into account the contract enforcement probability while the next hypotheses deal with group treatment comparisons.

Hypothesis 1. The frequency of trust and performance increases with a higher probability of contract enforcement.

A corollary to this is that the frequency of trust and performance decreases with a lower probability of contract enforcement. I expect Players 2 to choose perform with higher values of p as their expected payoffs from breaching the contract become lower. Anticipating this, Players 1 therefore choose to trust with higher values of p.

Hypothesis 2. The frequency of performance is higher with an in-group partner than with an out-group partner or with a partner whose group identity is unknown.

More players 2 will choose perform when paired with an in-group partner than when they are paired with an out-group partner or with a partner whose group identity is unknown. As shown in numerous experiments, the psychological costs of breaching an agreement is higher within a group than without a group.

Hypothesis 3. *The frequency of trust with an in-group partner is at least as high as the frequency of trust with an out-group partner or with a partner whose group identity is unknown.*

Unlike with the choice to perform by Player 2, the choice to trust by Player 1 is additionally affected by the parameter a or the costs she suffers if Player 2 breaches and is not found liable. If the psychological costs from breaching an in-group member δ_I is sufficiently higher than the psychological cost of breaching an out-group member δ_O, then trust choices can be more likely with an in-group than with an out-group. Otherwise, the costs suffered from unpunished breach, a, drives Player 1's choice to trust and also the

choice at which minimum level of contract enforcement probability she begins to trust. In this case there will be no difference in the likelihood of trust between group treatments.

Hypothesis 4. There is no difference in the frequency of trust and performance with an out-group partner and with a partner whose group identity is unknown.

The comparisons made thus far have been between the in-group treatment versus the two other group treatments. I predict that there will be no difference in the choices to trust and perform in interactions with an out-group partner and in interactions wherein the group identity of the partner is unknown.

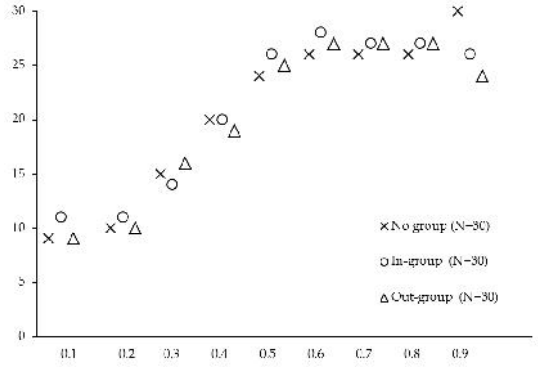
Although one can also consider the possibility of out-group derogation in which case the psychological costs from breaching an agreement with an out-group member is lower than with a stranger, I assume that psychological costs are equal. I make this assumption on the basis of experimental literature[21] showing that variations in bias in inter-group exchange relations arise due to variations in in-group favoritism more than because of variations in out-group derogation.

2.4. Results

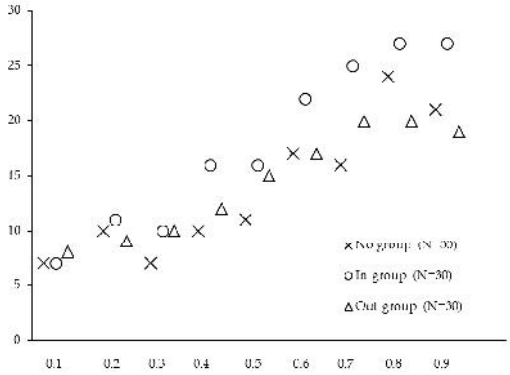
The results support Hypothesis 1 stating that trust and performance increase with a higher contract enforcement probability. The other hypotheses on the effect of the group treatments are also supported by the data.

Figure 2.2 shows the frequency of trust and perform choices for each probability condition and group treatment. An initial look suggests that as predicted in Hypothesis 1, trust and perform choices increases with probability condition. The differences in trust and perform choices between group conditions are not as easy to distinguish.

[21] See Balliet et al. (2015) and the literature cited therein.



The figure above shows the frequency of trust choices of 30 participants assigned as Player 1 in each group condition. Two participants in the no-group condition and one participant in the in-group condition consistently chose "not trust" in all probability conditions. One participant in the in-group condition switched their choice more than once.



The figure above shows the frequency of perform choices of the participants assigned as Player 2 in each group condition. Four participants in the no-group condition, 4 participants in the in-group condition, and 2 participants in the out-group condition consistently chose "perform" in all probability conditions. Six participants in the no-group condition and 1 participant in the in-group condition switched their choice to perform more than once.

Figure 2.2.: Frequency of trust and perform choices, by group and probability condition

2.4.1. Group identity

In this section, I focus on the effect of group identity on the choice to trust or perform. Table 2.2 shows the result of the tests. There are significantly more perform choices observed in the in-group condition than in the no group and out-group conditions and I find no statistically significant difference when comparing perform choices in the no group and out-group condition. I find no statistically significant differences when comparing trust choices between group conditions. The results of the Chi-squared tests support Hypotheses 2 that performance is more likely to occur with an in-group partner than with other types of partners.

Table 2.2.: Chi-squared goodness-of-fit tests on trust and perform choices, by group condition

	Trust		*Perform*	
Group condition	Pearson's χ^2	*p*-value	Pearson's χ^2	*p*-value
NG-IG	0.6976	0.404	10.7251	0.001
NG-OG	0.0761	0.783	0.3644	0.546
IG-OG	0.3131	0.576	7.1618	0.007

Note: I test the null hypothesis that there are no differences in trust or perform choices between group conditions. There are 30 participants in each group condition and each participant made 9 choices (N for each group is 180). I report the chi-squared statistic with 1 degree of freedom.

2.4.2. Group identity and contract enforcement

I further investigate how trust and perform choices differ across group conditions, even when controlling for third party contract enforcement quality. I estimate a panel binary choice model to test the relationship between trust, performance and institutions in the experiment. In the model, p_i is the probability of trust for first-mover participant i and performance for second-mover participant i. The dependent variables are P, the vector of contract enforcement dummy variables and G, the vector of group treatment variables. The regression also includes X, a vector of control variables that includes the participants' gender (Female=1, 0 otherwise), age, and nationality (German=1, 0 otherwise). The βs are log of the odds for a unit change in the explanatory variables while $u_i$is the error term.

$$\ln\left(\frac{p_i}{1-p_i}\right) = \beta_0 + \beta_1 P + \beta_2 G + \beta_3 X + u_i$$

The results of the regression are presented in Table 2.3. I performed 200 bootstrap replications to obtain normal-based 95% confidence intervals for the estimates which according to (Mooney and Duval, 1993) is adequate for normal-approximation intervals.

Table 2.3.: Logistic regression estimates on trust and perform

	Trust		Performance	
	β	z	β	z
Contract enforcement probability (baseline is p=0.1)				
0.2	0.1880	(1.05)	0.6422**	(2.50)
0.3	1.3681***	(3.73)	0.4140	(1.54)
0.4	2.4727***	(4.93)	1.2051***	(3.33)
0.5	4.011***	(4.94)	1.4718***	(2.87)
0.6	4.9131***	(4.88)	2.4004***	(3.98)
0.7	4.7331***	(4.48)	2.7548***	(3.91)
0.8	4.7330***	(4.07)	3.5746***	(4.38)
0.9	4.0107***	(3.97)	3.2217***	(4.04)
Group identity (baseline is no group)				
In-group	0.3426	(0.49)	1.0059**	(1.86)
Out-group	0.0659	(0.09)	0.1719	(0.30)
Participant variables				
Female	-1.316**	(-1.98)	0.6794	(1.37)
German	-0.6681	(-0.40)	0.1419	(0.34)
Constant	-0.0556	(-0.03)	-2.6572	(-3.36)
Log-Likelihood	139.40***		116.40***	
Wald χ^2 (12)	35.04***		24.78***	
N of observations	810		810	
N of groups	90		90	

Note: $***$ (<1%), $**$ (<5%), and $*$ (<10%) mark which variables were statistically significant in each model and indicate their associated levels of significance. The dependent variable takes a value of 1 if participant chose to trust or perform, 0 if not.

The regression results show that as predicted in first hypothesis, a higher probability of third party contract enforcement encourages both trust and performance. Shared group identity affects performance but not trust. The likelihood of performance is higher with an in-group partner than with an out-group partner or with a partner whose group identity is unknown but there are no statistically significant differences in trust choices based on the knowledge of partner's group identity. Females are also less likely to choose trust than males, but this effect is not observed with choices to perform. I also find no statistically significant difference in the choices between German and non-German participants. These findings are robust with the inclusion of answers to the post-experimental questionnaire which can be found in Table A.1 in Appendix A.2.

2.4.3. Discussion

In the exchange relations I mimicked in the laboratory via a contract game, two players can enter and fulfill a contract to produce joint surplus. To model varying quality in formal institutions, the probability that the contract will be enforced should the second player breach changes in each decision. As predicted, more contracts are offered and performed when the probability of contract enforcement is higher. To depict a type of informal institutions, I induce and manipulate group identity. In one treatment, players interact with an in-group member, in another they interact with an out-group member, and in a third treatment, they interact without knowing the group identity of their partner. In this variation, shared group identity increases contract performance but does not affect contract offers.

It is well-known that one way of reducing undesired behavior is to increase the probability that it will be sanctioned. However, under low probabilities of enforcement, people tend to rely on other means to enforce agreements and common group membership can be used to evaluate the probable trustworthiness of the other party. The results of the experiment show that shared group membership increases performance but not trust choices. Unlike Buchan et al. (2002) who found differences in trusting behavior between in-group and out-group members in a trust game, I do not observe a significant difference in trusting behavior as the context of the exchange moves from the in-group to the out-group. One crucial distinction though of this experiment from Buchan et al. (2002) is that participants played a standard trust game which involved no exogenous chance move after the second player made a decision.

That group membership only mattered for second movers corresponds to what Yamagishi et al. (1999) postulate: group membership may be relatively less important in first move cooperation in sequential cooperative decision-making tasks.[22] Because the contract game is sequential, the second player decides as if the first player already offered a contract and thus perhaps highlights the norm to reciprocate. Bicchieri et al. (2011) also finds a similar distinction related to behavior of first and second movers in their study using an incentivized survey. They asked participants whether they would punish untrusting investors (first movers) and untrustworthy performers (second movers) and find that most people would not punish untrusting investors, regardless of whether the potential trustee was a stranger or a

[22] See also Balliet et al. (2015) and Brewer (2008) for supporting experimental evidence.

friend. In contrast, most participants would punish someone who failed to reciprocate a stranger's or a friend's trust.

2.5. Conclusion

Formal and informal institutions shape and constrain behavior. Although much has been said about its role in shaping economic growth and exchange relations, it has only been recently that we have begun to understand how exactly they shape individual behavior. I conducted a laboratory experiment to distill the essential characteristics of formal and informal rules and disentangle the effects of varying qualities of institutions on behavior in an exchange relation. Using the contract game by Bohnet et al. (2001), I manipulated the quality of formal institutions by varying the probability of contract enforcement. I introduced informal institutions in the laboratory by induced shared expectations on behavior through common knowledge of shared group identity. Results show that the quality of formal institutions matter for contracts to be offered and fulfilled while informal institutions only matter for contracts to be fulfilled in one-shot exchange relations.

One of the crucial functions of institutions in an exchange relation is that they make the act of trusting or offering a contract less risky by aligning the incentives of both parties. In a one-shot anonymous interaction, the shadow of the courts (formal rules) matter given that the shadow of the future (repeated interaction) casts little or no shade.[23]

While the reliance on informal institutions in early exchange relations documented in Greif (1993) and Landa (1995) hinged on its reputation-building function for future interactions, the results on group identity manipulation suggest that shared group membership also helps in performance but not trust in one-shot anonymous interactions. People tend to perform more contracts with in-group members than with out-group members. However, the material costs of cooperation itself, an aspect that I control in the experiment, can also be affected by institutional quality and this may worthwhile to investigate in the future. Bolton et al. (2005) find that in transactions where the costs of cooperation are high, providing information about a part-

[23]The shadow of the courts is phrase I borrow from Brennan et al. (2003) while shadow of the future was coined by Axelrod (1984). Formal rules need not be a function of the courts or of governments only. In Güth et al. (2007), feedback mechanisms for (some one-shot anonymous) exchanges in online platforms such as eBay perform a type of third party enforcement.

ner's immediate past action increases cooperation. This may be something that is easier to accomplish for in-group members than out-group members, even when the quality of formal institutions is high.

It would also be interesting to see in future work how the relative importance of formal rules over informal constraints on behavior changes with repeated play. As Boettke et al. (2008), Bohnet et al. (2001) and Stiglitz (2000) argue, history matters for institutions. Bohnet et al. (2001) note that institutions shape behavior but by affecting behavior, they also affect preferences. In a similar vein, another question this study raises is how the quality of formal institutions at the beginning change preferences for exchanges within one's group in the long-run. The experiment design presented here can lend itself as a starting point to answer questions like this.

3. Does the gender mix among employers influence who gets hired? A labor market experiment

3.1. Introduction

Claudia Goldin (2014) writes in her presidential address to the American Economic Association that one of the grandest advances of the last century has been "the converging roles of men and women in society and the economy." The last fifty years have indeed seen significant progress in the educational attainment, political representation, and labor force participation of women (Pande and Ford, 2012). The United Nations Millennium Development Goals Report states that all developing regions have achieved, or were close to achieving, gender parity in primary education in 2012 (Millenium Development Report, 2014). By 1994, women had obtained the right to vote in 96% of the countries in the world (Ramirez et al., 1997). Growth in the labor force was higher for women than for men in every region in the world except Africa in the 1980s and early 1990s (Lim, 2009).

However, these overall improvements in female labor force and political participation have not yet translated into a corresponding increase of women in leadership positions. Less than 24% of legislators in parliaments around the world are female (Women in Politics, IPU and UN, 2015). In the corporate sector, female representation also declines with higher positions. To illustrate, only 17.8% of board members of the largest publicly listed companies in the European Union are women (European Commission, 2014), in contrast to a female labor force participation rate of 65.8% (World Bank, 2014)

One direct but still widely debated policy to address the marginal representation of women in leadership positions is the introduction of gender

quotas. The central idea behind quotas is that the proportion of women in decision-making bodies should not be lower than a certain level. In political representation, gender quotas can come in the form of having seats reserved for women in the parliament, imposing a minimum number of females in the candidate lists, or as measures written in the statutes of parties (Women in Politics, IPU and UN, 2015). In the corporate world, quotas come in the form of legislated ratios of female representation in the corporate board and / or senior management. Some countries that have enacted corporate board quotas include Norway, France, Belgium, and Canada.

Our paper investigates an assumption of the gender quota policy, namely that changes in the gender composition of a decision-making body can influence the individual decisions of its members. We investigate this in the context of hiring employees. Specifically, we consider whether the gender mix at the level of employers affects individual hiring decisions. We conducted a laboratory experiment where we matched two employers with two applicants, gave information about the level of competence, gender, age and education of the applicants to the employers, and observed consequences of changing the gender mix among employers on the number of males and females hired. We structured the pay-offs of employers and applicants to eliminate strategic and other-regarding concerns so as to focus on the effects of taste-based discrimination.

The next few paragraphs review related literature on gender quotas and hiring discrimination and outline our main contribution.

3.1.1. Gender quotas

A motivation for our work is an assumption implicit in the imposition of gender quotas in leadership positions, which is that higher representation of women at senior levels will foster the advancement of women at lower levels. Previous studies on gender quotas have mostly investigated its impact in terms of equity and efficiency goals such as achieving wage equality or improving company performance. Given this research focus, most research uses large data sets at the country- (Ahern and Dittmar, 2012) or at the firm-level (Chambliss and Uggen, 2000; Gorman, 2005). (Pande and Ford, 2012) however states that it remains difficult to make causal claims in terms of the impact of gender quotas on efficiency and equity. To illustrate, gender quotas are often legislated at the same time as the notion of equality in leadership and representation becomes more widely accepted (Krook, 2006). Changes in outcomes can thus be attributed to changing attitudes towards

female representation rather than to a quota policy *per se*. Another issue is that most gender quotas have only been implemented recently, and their effect on efficiency or equity might take a long time to be established. Our idea in this experiment is to consider how gender quotas affect individual decision-making. Underlying the discussion on gender quota's relationship with equitable or efficient outcomes is its impact on individual preferences and behavior. In this case, instead of using firm or country data, conducting experiments that allow us to investigate individual decisions might not only be more feasible, but also useful.

We focus on hiring because it is an observable outcome that can be directly linked with the gender quota policy to the extent that corporate board members or senior management are directly involved in hiring decisions. While discrimination in hiring has been investigated in many empirical and theoretical studies, few have related hiring discrimination to the gender composition of the pool of employers. The closest studies to ours in this regard are by Bagues and Esteve-Volart (2010), which uses a repeated randomized experiment to test if the gender composition of recruiting committees affects the chances of success of 150 000 female and male candidates for positions in the Spanish Judiciary, and by Zinovyeva and Bagues (2010) which also uses a randomized experiment to see how the proportion of female evaluators increases the chances of success of female applicants to full professor positions in Spain. Both studies find some evidence of a positive relationship between the proportion of female members in the pool of employers with the proportion of women being hired.

Following this line of research, we conduct a laboratory experiment with university students as participants to investigate discrimination at the individual level. We believe that the use of university students as participants is appropriate in this context given that they are expected to pursue careers in the corporate or policy sector and assume leadership positions. The laboratory environment allows us to control the composition of the pool of employers, the pairs of applicants that are presented to them, the task that applicants are asked to perform, and the information both parties have about each other. Thus, we avoid the identification challenge in some empirical studies that are caused by the distribution of applicants being skewed towards one gender, or the possibility that requirements of the position favor one gender over the other. In this regard, our work is related to Bendick Jr. et al. (1994) who use experiments to uncover racial discrimination in hiring and to Balafoutas and Sutter (2012) who test in the laboratory the effects of different policy interventions, one of which is quotas, on the likelihood that

women take part in competitions.

3.1.2. Statistical and taste-based discrimination

While we wish to investigate the role of the gender composition of the hiring pool in hiring decisions, we need to disentangle two types of discrimination, statistical and taste-based discrimination. Previous studies have documented those drivers of discrimination in the labor market.[1] Statistical discrimination (Arrow, 1973; Aigner and Cain, 1977) occurs as a result of imperfect information about the performance of potential candidates. One's social group, in this case gender, can be used as an indicator of performance instead of actual performance. To illustrate, male employers in a male-dominated occupation will favor males because they have relatively more information about male performance and thus perceive the choice of females as more risky. This risk aspect can drive the selection of a male applicant even against a female candidate that appears to be more competent. As males employers disproportionally select males, they continue to receive more information about male performance and less information about female performance, thus contributing to the persistence of a bias against hiring females. Male and female candidates then have different incentives to invest in human capital as the probability to get returns on their investments in a particular occupation depend on their gender (see also Beaman et al. (2009)). Some female applicants may then opt not to apply for male-dominated jobs, and those already employed in those jobs have less incentives to invest the time and effort to advance their position in the company. This mechanism may be one of the reasons that males maintain their representation in senior positions (Burgess and Tharenou, 2002).

Research in psychology has long investigated the many ways statistical discrimination manifests itself. In the workplace, for example, females are usually perceived as less competent and less productive than males (Huddy and Terkildsen, 1993). This stereotype persists even when presented with information that females are equally or more competent and productive than males. In a study by Steinpreis et al. (1999), identical scientific resumes were sent to 238 male and female academics. These resumes either have a male-sounding or female-sounding applicants' name. The academics were asked if they would accept the applicants as their working colleagues. Female and male academics accepted significantly more male applicants than

[1] see Darity and Mason (1998) for a comprehensive summary

female applicants. Moss-Racusin et al. (2012) also find similar results in their study. Moreover, they find that that perceived competence mediates hiring decision. That is, females are perceived as less competent and consequently, hired less often. They also found that females with an identical resume were hired with lower starting salary and with lower mentoring commitment.

Gender discrimination in the labor market can manifest itself not only because of differences in the quality and quantity of information employers receive on applicants, but also because employers have a preference for one gender (usually male) over another. This is what Becker (1971) modeled as taste-based discrimination.

Models of statistical and taste-based discrimination do not necessarily mean that individuals discriminate consciously. Indeed, Bertrand et al. (2005) focus rather on implicit attitudes as a driver of discrimination. In our experiment therefore, we not only controlled for statistical discrimination by asking participants whether they perceive males as higher performing than women in the task applicants were asked to perform for them, but we checked for the implicit attitudes of our participants. We implemented an Implicit Association Test ("IAT") similar to what Rooth (2010) did to analyze hiring discrimination among applicants with Arab-sounding or Swedish-sounding names. We also had participants answer a post-experimental questionnaire to check their explicit attitudes on gender to take into account possible discrepancies between overt expressions of hiring prejudice and implicit attitudes (Rudman and Kilianski, 2000).

The paper proceeds by describing the experiment design in Section 2. Section 3 discusses the hypotheses. Section 4 presents and discusses the results while Section 5 concludes.

3.2. Experimental design

We designed an experiment to test whether discrimination in hiring is taste-based as in Becker's model and whether such discrimination is mitigated by the gender composition of the pool of employer. We mimicked a labor market in the laboratory wherein there are two types of agents—applicants and employers. To limit the scope of statistical discrimination, employers in the experiment received direct information about the performance of applicants from both genders. We further discuss in this section details of our design. Instructions given to participants are shown in Appendix B.1.1.

3.2.1. Types of participants and tasks

Each session had with 30 participants:15 males and 15 females. We assigned 24 participants in the experiment to be employers and 6 to be applicants. Applicants were asked to performed a real-effort task while employers had to perform hiring among applicants. Once this was done, we administered an the Implicit Association Test (IAT) and asked participants to fill a post-experimental questionnaire. Table 3.1 gives a summary of how the experiment proceeded for participants.

Table 3.1.: Overview of experimental procedure

Applicant (A)	Employer (B)
Real Effort Task Round 1 (not remunerated)	Matched with another employer
	Information about applicants Decision who to hire
Real Effort Task Round 2 (payoff goes to applicant and, if this applicant is hired, also to the employer)	
	Implicit Association Test Questionnaire

Note: Participants at the beginning of the experiment were assigned either the role of applicant or employer. Applicants first perform a real effort task. Their performance here feeds into the information that is relayed to employers when they make a series of hiring decision. Applicants perform the same real effort task again which determines their earnings and the earnings of employers. Employers then answer an Implicit Association Test and a post-experimental questionnaire.

Real-effort task for applicants

Applicants in each session performed two rounds of a real-effort task that consisted of translating letters into numbers within a time limit. A crucial assumption of Becker's model to show the existence of taste-based discrimination is that potential employees in both groups are equally productive in their work. We thus needed a real-effort task that minimizes the differences in the actual performance among applicants across genders and in the beliefs of employers about differences in the competence of applicants at the task because of gender.[2] The decoding task in Kuhn and Villeval (2015) fit

[2]We wanted to avoid distinct gender differences in the attribution of performance like what Deaux and Eimswiller (1974) find, whereby the performance of a male on a task was more likely to be attributed to skill while an equivalent performance by a female

these criteria.

Figure B.1 shows a screen shot of the real-effort task. Displayed on applicants' screen was a table with two columns wherein the first column indicated letters and the second column indicated their corresponding numbers. At the right side of the screen was a letter which they have to convert to a number according to the table shown. After they entered a number, they confirmed their answer by pressing OK. A new conversion table was generated only if they correctly converted the letter to the corresponding number.

Based on their performance in a first round of the conversion task, applicants were grouped into three categories: low (first tercile), medium (second tercile), or high (third tercile). This performance grouping, together with their, gender, age, and education was communicated to employers in their hiring task. Whether they were hired or not, Players A performed a second round of the conversion task, which was remunerated 1 ECU (0.1 Euros) for each correctly converted letter.

Hiring task for employers

Employers in the experiment made a series of hiring choices between pairs of applicants. We presented 32 hypothetical pairs of applicants along with the 3 real pair of applicants to employers. Employers were informed about each applicants' age, gender, education, and performance in the first round of the real-effort task.[3] We showed information about the applicants' age and education to avoid triggering too much of a demand effect, whereby participants would realize that the experiment deals with gender discrimination. We also did so to better mimic the actual hiring process wherein employers receive other information than gender about an applicant. Figure B.2 in Appendix B.1.2 shows a sample of the screen employers encountered. We presented information about applicants jointly in pairs following work by Bohnet et al. (2012) which shows that joint evaluation reduces gender

on the same task was more likely to be attributed to luck. To check for this, the post-experimental questionnaire asked whether the participants believed males are better at the chosen real-effort task than females.

[3]Employers knew whether an applicant was a low, middle or high performer. To control for considerable differences in employers' interpretation of what low, middle, and high are, we informed them that those assigned to the group low could convert around 40 to 60 letters, those in the group middle could convert around 61 to 80 letters, and those in the group high could convert more than 80 letters. This information was based on a pilot session where another set of participants recruited from the same pool of university students were asked to perform the conversion task.

bias compared with separate evaluation. This enabled us to further isolate individual gender preferences as a source of hiring bias instead of other environmental factors.[4]

The task of the employers was to choose which applicant from each pair they would like to perform the same conversion task again for them, *i.e.* the applicant they hired would determine, through its (yet unknown) performance in the second round, the remuneration of the employer (employer and employee were paid the same).

As mentioned, employers encountered 32 hypothetical pairs, the same in all sessions. Mixed in this list were 3 real applicant pairs formed by matching the real participants in the same session. In total, employers encountered and decided for 35 pairs of applicants which were randomly presented across participants and across sessions. Employers did not know which of the pairs they encountered were real or hypothetical. Table 3.2 presents the characteristics of the hypothetical pairs in terms of gender composition. We balanced age and education in hypothetical pairs across sessions, *e.g.* if in one session applicant pair 31 was male of age 31 matched with female of age 35, then in the next session, applicant pair 31 was male of age 35 matched with female of age 31.

Table 3.2.: A breakdown of the 32 hypothetical applicant pairs

Applicant pairs	Male is higher	Equal rank	Female is higher	Total
Male-Female	4	12	4	20
Male-Male	4	2		6
Female-Female		2	4	6
Total	8	16	8	32

Note: There were 32 hypothetical pairs of applicants that were shown to participants B. We presented 12 male-female applicant pairs of equal ranks. There were also 8 other male-female applicant pairs, 4 of which had a higher-ranking male and another 4 had a higher-ranking female. There were 12 same-gender applicant pairs, 6 male-male pairs and 6 female-female pairs. Four male-male pairs and four female-female pairs had one higher-ranking member. Two male-male pairs were equally ranked and two female-female pairs were also equally ranked.

[4]However, we did not signal gender by using names. This is in contrast to previous hiring experiments wherein names were used to signal membership with a certain group. Rooth (2010), for example, conducted an experiment to test racial discrimination between Swedish- and Arab-sounding names. We avoided using names in this study because names themselves may be associated with certain stereotypes or groups apart from gender.

Implicit association test for employers

After the decision task, employers took part in an Implicit Association Test (IAT) which measured their implicit attitudes on gender and based on the work of Greenwald et al. (1998). We conduct an Implicit Association Test to control for implicit attitudes related to gender.

The IAT we implemented is a computer-based sorting task. The IAT consists of 7 blocks. In each block, the participant sorts words according to different categories on the upper portion of the screen. The sorting is executed by pressing either a right or left button. In the gender IAT we implemented, participants observe words related to male and female, words associated with warmth, and words associated with competence (Fiske et al., 2007). If, for example, one has an implicit view of females being warm, then sorting the words of female and warmth on the same side of the screen will be faster than sorting words of female and competence on the same side of the screen. If one has an implicit view of males being competent, then sorting the words of male and competence on the same side of the screen will be faster than sorting words of male and warmth on the same side of the screen. If there is no underlying association, then there should be no difference in the time it takes to sort (fe)male and warmth on the same side of the screen and the time it takes to sort (fe)male and competence together.

After completing the task, each employer receives an IAT score which is measured as follows: the average reaction time of sorting male words with competence and female words with warmth is subtracted from the average reaction time of sorting female words with competence and male words with warmth. A negative score implies that a participants sorts more quickly female words with competence while a positive score implies that an employer sorts more quickly male words with competence. A screen shot of the sorting task can be seen in Figure B.3 in Appendix B.1.2.

Post-experimental questionnaire for employers

We elicited employer's beliefs about gender differences in the experimental task and in other fields. This is to check for overt expressions of prejudice, if there are any. Other items include questions on trust, risk, and demographic characteristics. A copy of the questionnaire can be found in Appendix B.2.

3.2.2. Treatments, information, and earnings

We implemented a between-subject design by creating 3 types of employer pairs with varying gender composition: Male with Female, Male with Male and Female with Female. This determined four types of employers: a Male Employer who was paired with a Male Partner (ME-MP), a Male Employer who was paired with a Female Partner (ME-FP), a Female Employer who was paired with a Male Partner (FE-MP), and a Female Employer who was paired with a Female Partner (FE-FP). This means that a male participant assigned as an employer can be paired with a male or a female employer. Similarly, a female participant assigned as employer can be paired with a female employer or a male employer. Employers were informed of the gender of their partner in the pool of employers, and the identity (and thus gender) or their partner remained constant for the whole of the experiment (see Figure 3 B.2). Note that employers did not observe the decision of the other employer in their pool, and there was no communication between employers.

All employers underwent the same experimental procedure, the only difference in each treatment was the information they received about the gender identity of the other employer. In total, there were 144 employers in the experiment of which 72 were male and 72 were female. Table 3.3 summarizes the total number of participants assigned in each pairing condition across the six sessions of the experiment.

Table 3.3.: Number of participants, by gender composition of hiring pair

	Male Partner (MP)	Female Partner (FP)
Male Employer (ME)	36	36
Female Employer (FE)	36	36

Note: There were 144 participants B or employers in our experiment of which 72 were male and 72 were female. We matched participants B into pairs to form a pool of employers. A male participant can be paired another male participant or another female participant. Similarly, a female participant B can be paired with another female participant or a male participant. Each participant B however makes their hiring decisions individually.

Applicants were paid according to their performance in the second round of the conversion task, regardless of whether or not they were hired by an employer, and employers knew this. Employers were paid according to the performance of the applicant they chose in the real pair of applicant that was drawn at random at the end of the experiment. After employers made all their 35 decisions, the computer randomly selected which of the 3 real

pairs of applicants who were in the same session would be relevant for their earnings. The same pair was also the one that was relevant for their fellow employer. If both employers in the employer pair chose the same applicant, then the earnings of that applicant in the second round of conversion was also the earning of both employers. If employers hired different applicants in the pair, then one of the employers was selected at random to be the one who determines which applicant was hired, and the performance of that applicant was then the earning of both employers in the pair.

We made clear to all participants that applicants, whether they were selected by an employer or not, would earn the ECUs as per their performance in the second round of conversion task. This was to exclude issues whereby a lower performing applicant would be selected out of pity by an employer. Furthermore, although applicants were aware that their performance in the second round may affect the earnings of an employer, we did not inform them if they were selected by an employer and we did not give them any information about their employer. This was also known to employers.

3.2.3. Procedure

We recruited participants via ORSEE (Greiner, 2004). Fifteen males and 15 females were invited for each session of the experiment. Six participants (3 males, 3 females) were assigned to be applicants and 24 participants were assigned to be employers. There were six sessions of 30 participants each during the months of July and August of 2014. In sum, the experiment therefore had 180 participants, 144 of whom were employers and 36 of whom were applicants.

We framed the experiment in a neutral way: applicants were referred to as "Participant A" and employers as "Participant B." At the beginning of the experiment, we gave participants a copy of the instructions corresponding to their role (Appendices B.1.1 and B.1.2) and assigned them to a visually isolated computer terminal. All participants had to answer control questions designed to verify their understanding of the experiment. The experiment proceeded only once all had answered those questions correctly. The real-effort task for applicants was programmed using z-Tree (Fischbacher, 2007) while the hiring task and the IAT for employers was programmed in E-PRIME (Psychology Software Tools, Inc., 2012).

Participants were 26 years old on average (*SD:* 4.34). Most of them were students from the Friedrich Schiller University or of the University of Applied Sciences in Jena, Germany. Participants received 2.50 euros for show-

ing up on time and earned more depending on their performance and decisions in the experiment. Average payment in the experiment was 11.6 Euros and the experiment lasted one hour to one hour and a half depending on the session.

3.3. Hypotheses

We begin with a model of Becker (1971) which shows how an employer distaste for a certain group of employees may lower the number of such employees that are hired. The model assumes that all workers are homogeneous and equally materially productive and that the owners of firms are also the employers.

Hypothesis 1: There will be a difference between the hiring rates of male and of female applicants. This difference remains even between equally performing male and female applicants.

More specifically, we expect that male applicants will be chosen more than half the time by both male and female employers. This is because males are perceived to be more competent than females according to the empirical and experimental work cited thus far.

Hypothesis 2. Female applicants will be hired more than half the time in the FE-FP and FE-MP conditions and male applicants will be hired more than half the time in the ME-MP and ME-FP condition.

While the first hypothesis is about the proportion of male and female applicants hired in the aggregate, the second hypothesis is about the hiring decisions of employers at the individual level in each pairing condition. If employers manifest same-gender preference, then we expect that females will be hired more than half the time if the decider is female and male applicants will be hired more than half the time the decider is male.

There is some empirical evidence that point to this direction. Cohen and Huffman (2007) use nested data from the 2000 U.S. census to show that the greater representation of women in management narrows the gender wage gap and that the presence of high-status female managers has a significant and large impact on lowering wage inequality. Chambliss and Uggen (2000) find that minority partner representation has a positive effect on minority associate representation.

Hypothesis 3. The more females there are in the hiring committee, the more female applicants are hired.

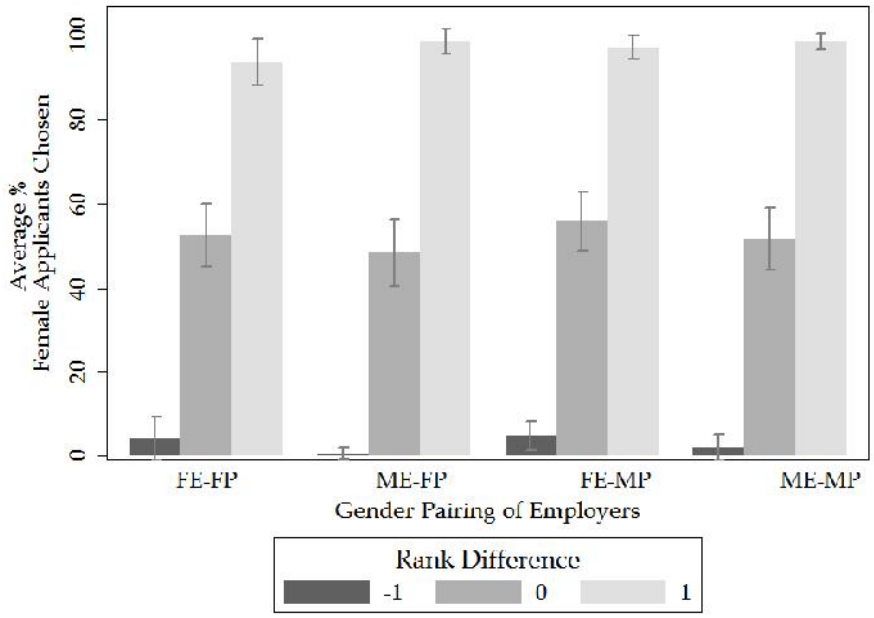
If the second hypothesis deals with the effect of each pairing condition on the hiring rate of female applicants, the third hypothesis compares the female applicant hiring rate across all pairing conditions. More specifically, we expect the most number of females hired in the FE-FP and least number of females hired in the ME-MP condition.

However, this relationship though may not be clear-cut and can be affected by other factors. For instance, Zinovyeva and Bagues (2010) find that the effect depends on the position at stake. An additional woman in an evaluating committee with seven members increases the number of women promoted to full professor by 14% on average. They also find that there is no significant relationship between the gender of evaluators and the gender of hires in the case of associate professors. Similarly, Gorman (2005) finds in her study using 1990s data from large U.S. law firms that Female decision-makers fill more vacancies with women than do male decision-makers but only among entry-level hires.

3.4. Results

We find no significant difference in the aggregate hiring rates between male and female applicants contrary to what was predicted in Hypothesis 1. However, we find some evidence in support of Hypothesis 2 showing that female employers with a male partner hire female applicants more than half the time than equally-performing male applicants. Hypothesis 3 is also not supported by our analysis: once we control for employer characteristics, we do not find any statistically significant difference in hiring choices across the four pairing conditions.

We begin our analysis of employers and their decisions with some descriptive statistics in Figure 3.1 and in Table 3.4. A further analysis of experimental data from applicants is shown in Appendix B.3.



Note: The figure above shows the mean percent of female applicants chosen across the four employer pairing conditions. The whiskers of the bar indicate the 95% confidence interval of the mean. A rank difference of -1 means that the male applicant had a higher rank, a rank difference of 0 means that both have the same rank, and a rank difference of 1 means that the female applicant had a higher rank.

Figure 3.1.: Mean percent of female applicants chosen, by rank difference and gender pairing of employers

Figure 3.1 decomposes the hiring decisions per employer on the 20 male-female applicant pairs by rank difference and by gender pairing treatment. Rank was numerically translated so that those with rank "High" had a rank value of 3, those with rank "Middle" had a rank value of 2, those with rank "Low" had a rank of 1. We compute rank difference by subtracting a male applicant's rank from a female applicant's rank. Thus, a rank difference of -1 means that the male applicant had a higher rank, a rank difference of 0 means that both have the same rank, and a rank difference of 1 means that the female applicant had a higher rank. The graph shows that the better performing an applicant is, the more likely she is hired.

3.4.1. Aggregate hiring rates of male and female applicants

Part 1 of Table 3.4 shows the average proportion of females hired, by employer on the hypothetical male-female applicant pairs. Note from the previous section that each employer encountered 20 hypothetical pairs of this kind. In all treatments, we observe that employers chose females roughly half the time. A chi-squared goodness of fit test shows that the proportion of hired female applicants is not significantly different from 50%, ($\chi^2(1, 2880) = 2.11$, $p = 0.15$).

We therefore cannot reject the hypothesis that there is no significant difference in the aggregate hiring rates of male and female applicants. Although there were extreme choices in individual hiring with one employer hiring as few as 4 women overall (20% M-F applicant pairs) and 2 others hiring as many as 16 women overall (80% M-F applicant pairs), the distribution of female applicant choice did not significantly differ between genders. The median number of women hired by a male employer and a female employer was 10, i.e. women applicants were hired around 50% of the time.

Even when we limit our analysis to hiring decisions on equally performing applicants, we still find that the proportion of hired female applicants is not statistically significantly different from 50%, ($\chi^2(1, 1728) = 3.52$, $p = 0.06$).

Table 3.4.: Summary statistics

Variable	Mean	SD	Min	Max	N
1. Hiring decisions on M-F Applicant Pairs					
All treatments	51.35	13.49	20	80	144
FE-FP	51.11	13.37	25	80	36
ME-FP	49.08	13.98	25	80	36
FE-MP	54.02	12.97	25	75	36
ME-MP	51.25	13.69	20	70	36
2. Hiring decisions on M-F Applicant Pairs of Equal Performance					
All treatments	52.26	21.73	8.33	100	144
FE-FP	52.55	21.53	8.33	100	36
ME-FP	48.61	23.19	8.33	100	36
FE-MP	56.02	20.66	8.33	91.67	36
ME-MP	51.85	21.74	8.33	83.34	36

Note: The table above shows the summary statistics for the decisions made, IAT scores, and answers to the post-experimental questionnaire. The four treatments are labeled by gender (M for male, F for female) and their role (E as employer, P partner). For example, the hiring decisions of a male employer (ME) matched with a male partner (MP) will be counted under ME-MP. Part 1 is about the percent of female applicant chosen by each employer in the 20 male-female applicant pairs. Meanwhile, Part 2 is the percent of female applicant chosen by each employer in the 12 male-female applicant pairs of equal performance.

3.4.2. On hiring choice and gender mix of the pool of employers

The evidence so far shows that aggregate hiring rates do not differ between male and female applicants. We now begin our analysis on individual hiring choices and see if Hypothesis 2 holds.

Part 1 of Table 3.5 shows the results of our tests on the second hypothesis. Because we are testing whether the observed proportions for female applicant choice for each pairing condition differ from our hypothesized proportion of 50%, we use chi-squared goodness-of-fit tests. We find that the proportion of hired female applicants differ from 50% only in the condition when female employers are matched with a male partner. In other conditions, we do not find any statistically significant difference.

Table 3.5.: Chi-squared goodness-of-fit tests on female choice in male-female applicant pairs, by employer-partner pair

	1. All		*2. Equal Performance*	
Employer-Partner Treatment	Pearson's χ^2	*p*-value	Pearson's χ^2	*p*-value
FE-FP	0.36	0.71	1.12	0.29
ME-FP	0.27	0.60	0.33	0.56
FE-MP	4.67	0.03	6.26	0.01
ME-MP	0.45	0.50	0.59	0.44

Note: We test the null hypothesis that the percent of female applicant chosen = 50%. We first ran tests on hiring choices on all male-female applicant pairs (N for each treatment is 720) and then on hiring choices on male-female applicants pairs of equal performance (N for each treatment is 432). We report the chi-squared statistic with 1 degree of freedom. Given the results, we reject the null hypothesis that the proportion of female chosen is 50% in the condition where a female employer is paired with a male partner both when deciding for male-female applicants pairs and when deciding for male-female applicant pairs of equal performance.

Part 2 of Table 3.4 deals with the case of equally qualified applicants. Once again, we find that the mean proportion of female applicants is not significantly different from 50%, except in the FE-MP condition (56.02%) (Part 2 of Table 3.5).

3.4.3. On hiring choice and measures of implicit and explicit attitudes

We now proceed with a test on Hypothesis 3 and compare female applicants hired across the different gender mix of employers while controlling for implicit and explicit attitudes.

Table 3.6 shows summary statistics of the IAT score and answers to the post-experiment questionnaire. We see that male employers were slower when it comes to associating females with warmth and males with competence than female employers (mean response time of 101.89 ms *vs.* a mean response time of 83.70 ms for females).

Table 3.6.: Mean IAT score and answers to post-experimental questionnaire, by gender of employers

	Overall *N=144*	Male *N=72*	Female *N=72*	95% CI
IAT Score (in ms)	92.80	83.70	101.89	[90.30, 95.29]
Females are better at conversion task	0.29	0.24	0.33	[-0.05, 0.24]
Males are better at conversion task	0.24	0.19	0.29	[-0.04, 0.24]
Deliberately chose females	0.36	0.27	0.44	[0.01, 0.32]
Deliberately chose males	0.20	0.18	0.22	[-0.09, 0.17]
Male and female performance vary in some fields	0.87	0.85	0.88	[-0.07, 0.15]
...differ in medicine	0.20	0.22	0.18	[-0.17, 0.09]
...differ in law	0.22	0.25	0.19	[-0.19, 0.08]
...differ in science	0.25	0.26	0.24	[-0.17, 0.11]
Experimenters will respect privacy of participants	0.79	0.78	0.81	[0.04, 0.30]

Note: The IAT Score is the average reaction time of sorting male words with competence and female words with warmth and is subtracted from the average reaction time of sorting female words with competence and male words with warmth. All the other variables are binary variables, with a value of 1 for "Yes" responses and 0 otherwise. The reported 95% confidence intervals are from Fisher's exact tests for categorical variables except for the reported interval in the IAT score which is not categorical. For the IAT, we report the confidence interval from a two independent sample t-test.

We focus on seven questions in our post-experimental questionnaire that deal directly with employers' gender attitudes. We also asked participants if they believed that there are some fields where males and females differ in performance and questions about specific fields, namely, medicine, law, and science.[5] We also include a question on whether participants believed that their decisions will be kept anonymous to check for possible experimenter

[5]These fields were chosen on the basis of previous research on the existence of hiring discrimination in these fields

demand effect. The responses were coded into binary variables with "Yes" taking a value of 1 and other responses taking a value of 0. The values in Table 3.6 apart from the IAT score show relative frequencies of "Yes" responses and the 95% confidence interval of results from Fisher's exact tests on the difference between male and female responses.

We now consider whether IAT scores and answers in the questionnaire are associated with hiring decisions. Our data consists in a series of decisions by employers over a given set of applicant pairs. We therefore estimate a random effects logistic regression model as per equation 3.1 below:

$$P(y_{ij} = 1|X_{ij}) = L(X_{ij}\beta + u_i) \tag{3.1}$$

y_{ij} takes value 1 when employer i chose a female applicant when considering pair of applicants j and X_{ij} is the set of independent variables. As our main treatment variables, we include dummy variables capturing the employer-partner condition. For control variables, we consider individual characteristics such as age, the IAT score, and answers to the post-experimental questionnaire. We also include the characteristics of the applicant pair encountered. The last term u_i are employer-specific random effects.

Table 3.7 reports the results of the regressions. We performed 200 bootstrap replications to obtain normal-based 95% confidence intervals for our estimates. This is adequate for normal-approximation confidence intervals (Mooney and Duval, 1993).

We find that once we control for applicant characteristics, differences in applicant performance, and implicit attitudes and explicit beliefs, there is no statistically significant effect of employer-partner treatments on the decision to hire a female applicant in our regressions. The IAT score is also not significantly associated with choosing a female applicant. However, employers who believed that females are better at the real-effort task and who answered that they deliberately chose females were more likely to choose females.[6] Employers who answered that they believe that there are differences between male and female performance in some fields and in law and medicine were also less likely to choose female applicants. The 80% of participants who trusted the experimenters to not misuse their data were also

[6]We compute the difference between the answer to the question about whether females are better at real-effort task (respectively participant deliberately chose females) and whether males are better at real-effort task (respectively participant deliberately chose males).

more likely to choose males suggesting that some demand effect against discrimination for those participants who maybe expected experimenters to be able to relate their decisions to their name.

Table 3.7.: Logistic regression estimates of a model of choice of female applicant in male-female applicant pairs

	All M-F applicant pairs		Equal ranked M-F applicant pairs	
	β	z	β	z
Characteristics of applicant pairs				
Rank Difference (F - M)	3.79***	(12.94)		
Age Difference (F - M)	0.09***	(3.34)	0.07*	(2.14)
Education Difference (F - M)	-0.60***	(-8.55)	-0.62***	(-7.37)
Employer-Partner Treatments (baseline is ME-MP)				
FE-FP	0.17	(1.11)	0.22	(1.35)
ME-FP	0.11	(0.58)	0.14	(0.69)
FE-MP	0.13	(0.82)	0.12	(0.63)
Individual variables				
IAT Score	0.00	(0.07)	-0.00	(-0.17)
Females are better at task - Males are better at task	0.42**	(2.85)	0.49**	(3.18)
Deliberately chose female - Deliberately chose male	0.62***	(4.93)	0.65***	(5.25)
Male and female performance differ in some fields	-0.52**	(-2.59)	-0.46*	(-2.02)
...differ in medicine	-0.37+	(-1.75)	-0.32	(-1.54)
..differ in law	0.29+	(1.90)	0.33+	(1.88)
..differ in science	0.18	(1.25)	0.21	(1.36)
Experimenters will respect privacy of participants	-0.17*	(-2.25)	-0.22*	(-2.56)
Constant	0.51*	(2.11)	0.50*	(2.02)
Log-Likelihood	-1201.79		-1040.76	
Wald χ^2 (14)	239.86***		180.18***	
AIC	2439.692		2117.395	
N of observations	2880		1728	
N of groups	144		144	

Note: $***$ (<0.001), $**$ (<0.01), $*$ (<0.05), and $+$ (<0.10) mark which variables were statistically significant in each model and indicate their associated levels of significance. The dependent variable is female applicant choice which takes a value of 1 if a female is chosen, 0 if male. z statistics in parentheses are based on bootstrapped standard errors (200 replications)

3.4.4. Discussion

In the two-person pool of employers we formed, we find that female employers who are paired with male employers are more likely to choose a female applicant over an equally competent male. However, a higher number of females in the hiring committee does not necessarily lead to a higher number of female applicants being hired. While our experiment does not fully account for the mechanisms behind this effect, it does help us identify which explanations are more likely. One possible explanation from psychology is in-group bias or in group favoritism (Tajfel and Turner, 1979; Turner and Reynolds, 2010). This is the preferential treatment that is given to others by virtue of having the same group membership. When a female employer is paired with a male employer, one's in-group identity (female) becomes salient in relation to an out-group which is male. This activates in-group favoritism, resulting in female employers choosing more female applicants than male applicants.

In-group favoritism is stronger when the group is threatened as seen in the work of Steele and Aronson (1995). As females are less likely to be in decision-making positions in real life, being a female employer paired with a male employer reminds one of one's minority status. This is in contrast to the condition wherein female employers were paired with fellow female employers, wherein both females occupy decision-making positions. As Tajfel and Turner (1979) conceptualize, in-group bias is an effect that arises from a desire to maintain positive self-esteem or self-worth. When individuals' self-esteem is threatened, as in the case when they are in a discriminated group, individuals turn to group membership to protect their self-worth. Males, who are more likely to be in decision-making positions in real-life, perceive less of a threat from being paired with a female employer and therefore choose females as often as males. That our manipulation on the gender composition of employers affects hiring decisions by females and not by males is also consistent with experimental evidence by Azmat and Petrongolo (2014) suggesting that women are more sensitive to social cues.

The IAT scores of employers in Table 3.6 reflect pervading gender stereotypes: employers were quicker to associate female with warmth and males with competence than female with competence and men with warmth. This is consistent with the bulk of past research findings on gender-IAT and IAT in general (Fiske et al., 2007).

Moreover, implicit attitudes did not translate into a hiring bias while ex-

plicit attitudes did. This finding makes sense when we examine hiring as a deliberate process. The message from implicit and explicit literature indicates that explicit attitudes manifest their influence in conscious behavior while implicit attitudes manifest themselves in spontaneous behavior (Dovidio et al. (2002); Jellison et al. (2004); Rydell and McConnell (2006)). In this regard, our results are consistent with this message as we put employers in a situation where their decisions were of direct consequence to themselves and information was available to guide their decisions.

Our results also fit with what **?** find in their use of the IAT to measure ethnic bias in Africa: IAT scores report smaller magnitudes and statistical strength of bias relative to what is found in self-reported views from surveys. It appears that bias tends to be more strongly expressed when self-reported than when measured implicitly. In sum, while we observe preferential hiring for female applicants only when females are paired with males, the effect of gender composition and also implicit attitudes pale in comparison with explicit attitudes when it comes to hiring choice.

3.5. Conclusion

To say that gender discrimination exists is nothing new. However, previous studies have offered little evidence on what can mitigate hiring discrimination at the level of individual decisions or how perceived discrimination affects choices and behavior. Our work aims to help fill this gap in research and also in the discussion of policies geared towards increasing female representation in decision-making positions.

The relationship between increasing females in decision-making positions and increasing females in entry-level hires is rather complicated given that individuals are influenced by changes in conditions in which they make their decisions. Within the confines of the laboratory, establishing a causal relationship between the increased female representation in the pool of employers and the number of female hires becomes less complex. In this paper, we present a laboratory experiment that depicts a hiring situation wherein markets are competitive and information asymmetries between male and females do not exist.

We created two-person employer pools wherein employers received information about an applicant's competence that was directly related to the job that was to be performed. Employers' profits in this experiment were only determined by the applicant hired and they did not incur any other

costs in hiring. Applicants not hired could still earn income, hence there was little reason for our participants be concerned about a rejected applicant's welfare. The hiring decision of both employers was also independent, so there was no reason for an employer to favor an applicant to counteract the expected bias of the other employer. We only varied the gender of the other employer in the pool of employers. Under such conditions, the rational decision of an employer faced with the choice between two applicants is to hire the better performer. When making a choice between two equal performers of different genders, there was no reason to prefer one over the other.

The results of our experiment show that female employers when paired with male employers are more likely to choose a female applicant over an equally-competent male applicant. Although employers' implicit attitudes reflected prevailing stereotypes on males being associated with competence and females with warmth, we find that explicit beliefs and not implicit attitudes to be significantly associated with hiring choice.

Our experiment provides evidence that observed hiring discrimination can indeed be partially attributed to explicit individual preferences and influenced by changes in the gender composition of the pool of employers. It should be noted here though that the pool of employers in actual hiring situations rarely receive information about applicants and make hiring decisions the way employers do in our experiment. We eliminated information asymmetries between males and females and between applicants and employers in our experiment, which made discrimination less likely.

Our result on the role of explicit attitudes points to the role beliefs play in discrimination. It would be interesting to see whether such discriminatory beliefs persist with continued exposure to information on the equal productivity of two groups. To some extent, beliefs about their discriminated status may also have played a role on why females choose more females when paired with a male partner.

Further research

Our experiment laid the groundwork for further experiments that would dissect and separate, as we did, different channels for discriminating behavior to arise. Further research is still needed in order to explore how broad policies meant to increase female representation in the workforce can address not just market inefficiencies but can also have an impact on individual choices and preferences.

Our study also highlights the importance of understanding how individuals respond to perceived discrimination and to policies meant to address actual discrimination. Our study focused on employers' decisions but further research could also consider applicants behavior. Can changes in the gender composition of management prevent qualified applicants from censoring themselves from applying? Do applicants hired under a quota policy perform any differently than applicants hired under a no-quota policy? Despite the great strides made by women in the last 50 years, it is indeed still vital, and interesting, to further understand how individuals behavior and institutional policies interact to hinder or encourage women's involvement and success in the labor force.

4. On the malleability of fairness ideals: Order effects in partial and impartial allocation tasks

4.1. Introduction

People care about fairness but they also care about their own interest. In some instances, tensions can arise between the pursuit of what is thought to be fair and what could benefit oneself. When such tensions arise, how one defines fairness can become ambiguous, even malleable. People may appeal to different definitions of fairness depending on the material consequences of adhering to them.

Adam Smith (1759) in The Theory of Moral Sentiments[1] proposes an idea of fairness that can be characterized by an impartial spectator who has no personal interest in the outcome. This theory guided several approaches to empirically measure an objective definition of fairness.[2] The impartial spectator though is not only useful in terms of objectively defining fairness in interactions with other people, but also in terms of assessing moral conduct within oneself.

The evidence so far on the research on people's notions of fairness depict a range of complex motivations behind people's allocation decisions beside pure self-interest and pure altruism. According to Andreoni and Bernheim (2009), individuals care about fairness, but they also care about their social image by displaying a preference for being perceived as fair by others. Moreover, they also care about their self-image and perceiving themselves as fair. Indeed underlying the impartial spectator theory is also a concern for one's self-image.[3] To further illustrate, von Hippel and Trivers (2011) find

[1] See Part III, Chapter I "Of the Principle of Self-approbation and of Self-disapprobation", No. 2.

[2] See Konow (2000) and Aguiar et al. (2013) for examples.

[3] As Adam Smith (1759) writes in the same section, "We endeavour to examine our own

that individuals do not just adjust their actions to their principles but also try to convince themselves that serving their own interests do not violate their principles. Such efforts to maintain positive beliefs about oneself are in line with what Bénabou and Tirole (2011) identify as history-dependence in human behavior that can come in the form of self-signaling, i.e., attempts to influence the beliefs and actions of a future self by the present self and which can arise as people infer who they are on the basis of past actions.

In this study, we investigate in the laboratory how people's notions of fairness change in two allocation tasks that differ in whether participants have a stake in the outcome of their decision. In one task, they decide as partial stakeholders and in another they decide as impartial stakeholders. We explore two forms of history dependence in the decision of participants. First, we change the order in which participants undertake two allocation tasks, and second we vary their previous experience in allocation experiments in the laboratory.

This study provides a sharper focus on the role of self-image in explaining the flickering nature of people's fairness ideals. This is in contrast to previous research that emphasize the role of social image and other-regarding concerns. Previous studies analyzing fairness ideals have used the dictator game where a participant in the role of a dictator splits resources between herself and another player who can only accept the division. Camerer (2003) and Engel (2011) find that the average amount shared is over 20 % which goes against the theoretical prediction that a rational, purely self-interested player should keep the whole endowment to himself. However, when given the opportunity to obscure the relationship between their allocation decisions and final outcomes, dictators also behaved less generously as Dana et al. (2007) and Becker (2013) show.

By focusing on self-image, we argue like Bénabou and Tirole (2011) that self-image plays a fundamental role in explaining moral and pro-social behavior. Apart from a concern for others and what others think, people have imperfect knowledge about their own preferences. When asked to decide on matters related to matters such as fairness, they look at their past actions and attempt to maintain as much as possible a positive view of themselves.[4]

conduct as we imagine any other fair and impartial spectator would examine it. If, upon placing ourselves in his situation, we thoroughly enter into all the passions and motives which influenced it, we approve of it, by sympathy with the approbation of this supposed equitable judge. If otherwise, we enter into his disapprobation, and condemn it."

[4]Fairness and self-image awareness and manipulation are identified by ethnographers as

This could help explain why Rustichini and Villeval (2014) find in their experiments on hypocrisy, power, and social preferences that instead of simply acting according to their self-interest, people attempt to reconcile norms of fairness with the temptation to act selfishly so they need not alter their perception of themselves as fair. This could also better explain why there are still positive offers observed even when decisions in dictator game are anonymous. In this regard, our study is also related to the experimental work of Grossman (2010) who uses dictator-game experiments to separate the effect of self-image from social image on giving. In addition to what Grossman (2010) did, this study also looks at how participant's previous laboratory experience can alter these effects.

Overall, our findings suggest that some form of history dependence influences allocation decisions of participants. More specifically, we find that participants are less prone to behaving selfishly in the partial allocation task if they have decided impartially beforehand and have not yet participated in an allocation experiment.

The paper proceeds as follows. Section 2 describes in detail the experiment design and section 3 presents the hypotheses. Section 5 presents and discusses the results of the experiment while section 6 concludes.

4.2. Experimental design

In this section we present our experimental design and discuss the treatments implemented. Each participant performed two main allocation tasks: one as a partial stakeholder and another as an impartial spectator. They decided either partially first then impartially, or impartially first then partially. There were also two types of participants in the experiment. The first type were inexperienced participants or those who have not yet participated in any allocation experiment before and the second type were experienced participants or those who have participated in at least one allocation experiment before.

"human universals", i.e., "features of culture, society, language, behavior, and psyche for which there are no known exceptions." Brown (1991) compiles the full list of identified human universals.

4.2.1. Tasks

Participants in the experiment did four tasks: a real effort task, two allocation tasks and a guessing task. At the end of the experiment, participants in all treatments completed a guessing task based on a vignette.

Real effort task *(RE)*

The task consisted of counting zeros in a series of 5x5 tables for 5 minutes. Participants faced one table at the time on their computer screen (an example is shown in Figure C.1). If they correctly counted the number of zeros in the table, they received 1 experimental currency unit (ECU) and a new table was generated. In case of a mistake, they had a maximum of three attempts to provide a correct answer, otherwise no ECU was earned for that table and a new table was automatically generated. Participants had the chance to practice the task for two rounds. We made clear in the instructions that the performance in the practice rounds had no bearing on subsequent rounds or in the final payment.

The real effort task aimed to induce in participants entitlement over the share of resources they produced and to make an idea of fairness based on the proportionality principle salient. Homans (1958) and Rotter (1966) describe the proportionality principle as based on the idea that people's entitlement should depend on factors within their control (e.g. effort) and should not be affected by factors outside their control (e.g. luck).[5]

Another purpose of the real effort task was to create an amount of resources that measured the individual effort exerted. Before the start of the real effort task, participants already knew that the amount of resources generated in the first stage were the resources to be divided in the subsequent allocation task. We also gave them details about the first allocation procedure before the real effort task. We adopted the counting zeros task because it offered a reasonably clean measure of individual effort, a factor likely to be within the participants' control, and made an idea of fairness based on the proportionality principle salient.

[5]Using the proportionality principle may constrain the kind of fairness principles we can test in subjects' allocation decisions. However, it served two important functions in light of our research question: it provided an experimentally verifiable measure of fairness and self-interest and it allowed us to connect our work with previous experimental studies of fairness in the laboratory using the dictator game. We checked for other possible types of fairness principles of the participants through a guessing task on a vignette at the end of the experiment.

Partial allocation task (*P*)

This task proceeded like a standard dictator game. Participants in this allocation task decided how to allocate the total amount of ECUs earned in the real effort task by themselves and by another participant, while knowing the contribution of each other to the total amount to be allocated. Therefore, they faced a trade-off between applying the proportionality principle—that would prescribe an allocation which reflects the inputs given by each participant—and acting on their self-interest—that would dictate allocating to themselves more than they actually contributed.

Impartial allocation task (*I*)

Participants in this allocation task decided how to distribute the total amount of ECUs earned in the real effort task by two other participants. Each participant was a dictator for two other participants and her decision for those two others did not affect her own earnings. As the impartial spectator, she received the earnings she obtained in the real effort task. In this task, participants could apply the proportionality principle without any conflict with their self-interest.

Guessing task

At the end of the experiment, we presented a vignette from Krupka and Weber (2013) describing a situation similar to the one encountered in the two allocation tasks previously described. The task was conducted to check whether participants hold a different type of fairness principle as the social norm.

A total amount of 100 ECUs had to be allocated between Player 1 and Player 2 who contributed 40 and 60 ECUs, respectively. We asked participants to guess how socially appropriate the majority of participants would consider 11 different allocations. A 4-point scale with the following values was used: "Socially very unacceptable," "Socially quite unacceptable," "Socially quite acceptable," "Socially acceptable." The 11 potential allocations was obtained by starting from an allocation of 0 to Player 1 and of 100 to Player 2 and increasing (decreasing) in steps of 10 the allocation to Player 1 (Player 2).

4.2.2. Treatments

The treatments in the experiment were implemented between-subjects and varied i) the order of the partial stakeholder and impartial spectator allocation tasks and ii) the experience of participants with the allocation task.

Order of allocation tasks

Depending on the order in which the tasks are presented, we refer to two treatments: Partial-Impartial (PI) and Impartial-Partial (IP).[6]

We presented the experiment to participants in two parts. In the first part, participants, regardless of the treatment, did the real effort task. In treatment PI, they then proceeded with the partial allocation task. After the first part of the experiment is concluded, we informed participants of the details of the impartial allocation task and then participants made their allocation. In treatment IP, we reversed the order of the two allocation tasks. Participants first decided as impartial spectators and then as partial stakeholders.

Experience in allocation experiments

In addition to manipulating the sequence of the two allocation tasks, we also varied participants' previous experience in other allocation experiments in the same laboratory.Levin et al. (1988) note that experience in the laboratory helps participants to focus on the relevant factors in the experiment (e.g. payoff maximization) and less attention to peripheral factors (e.g. the order of the decision task).

The second treatment variation depended on whether or not participants previously participated in at least one dictator or ultimatum game experiment, according to the information stored in the ORSEE database of the laboratory where the experiment was conducted. In this treatment variation, we invited two different groups of participants to the lab: the experienced group (Exp) and the inexperienced group (Inexp). We administered the experiment to the groups in separate sessions (between-subject design). In both treatments, we followed the procedures commonly used in the laboratory and did not inform invited students of the type of experiment they were going to perform and of the previous laboratory experience of the others.

[6]The IP treatment replicated the complex dictator treatment in the experiment by Konow (2000).

Table 4.1 summarizes our four treatment variations.

Table 4.1.: Treatments, by role and experience

Treatments	Order of Allocation Tasks			Experience
	Part 1		Part 2	
	Stage 1	Stage 2		
IP-Exp	Real effort	Impartial	Partial	Experienced
IP-Inexp	Real effort	Impartial	Partial	Inexperienced
PI-Exp	Real effort	Partial	Impartial	Experienced
PI-Inexp	Real effort	Partial	Impartial	Inexperienced

Note: We employed a between-subject design which varied the order of the allocation task (IP or PI) and the composition of participants with regards to previous laboratory experience in allocation experiments (Exp or Inexp).

4.2.3. Procedure

We programmed the experiment using z-Tree by Fischbacher (2007) and we conducted 2 sessions of each treatment at the experimental laboratory of the Max Planck Institute of Economics (Jena, Germany), from July to September 2013. As displayed in Table 4.2, there was a total of 180 participants in the four treatments, with 21 or 24 individuals who took part in each session. In all treatments, participants were students from the Friedrich Schiller University in Jena, recruited via the ORSEE software by Greiner (2004).

Upon arrival at the laboratory, each participant was randomly assigned to one visually isolated computer terminal. It was common knowledge that the experiment was composed of two parts. First, each participant received written instructions for the first part of the game. They were also informed that the experiment consisted of two parts and the details of the second part would be revealed once the first part was finished.

We then read the instructions aloud for the first part and participants answered a set of control questions on the screen to check if they understood the instructions correctly. After the completion of the first part, we distributed and read aloud instructions on the second part of the experiment.[7] Each session lasted about 50 minutes and the average payment was 12.85 Euros (min 2.5, max 28.6), including a show-up fee of 2.5 Euros and the earnings obtained in the vignette study. The conversion rate of ECUs to Euros was 1 ECU = Euros 0.30

[7] All instructions were in German; an English translation can be found in Appendix C.1.

Table 4.2.: Number of participants per treatment

	PI	IP	PI + IP
Inexp	42	42	84
Exp	48	48	96
Inexp + Exp	90	90	180

Note: Eighty-four participants had no prior experience in allocation experiments and 96 had experience. Ninety participants decided partially first then impartially (PI) and 90 participants decided impartially first then partially (IP).

In both allocation tasks, all decisions were anonymous and no participant ever learned with whom they have been paired with in each decision. Moreover, we made clear to participants that all pairs formed during the experiment were perfectly independent from one another. Thus, participants never interacted with the same person twice in both decision tasks. We employed this matching procedure to avoid further confounding strategic considerations in participants' allocation decisions.

We informed participants that a random draw at the end of the experiment will determine which of the two experimental parts was going to be relevant for the payment. Since all participants decided both as partial stakeholder and impartial spectator, a second random draw determined whether a participant's allocation decision was going to be implemented.[8]

4.3. Hypotheses

Our experimental design allows us to test hypotheses on decisions as a partial stakeholder and their adherence to the proportionality principle. The first hypothesis compares decisions as partial stakeholder with decisions as impartial spectator. The second hypothesis considers the effect of the order manipulation while the third hypothesis deals with with the effect of prior laboratory experience.

Hypothesis 1: When deciding as partial stakeholders, participants will allocate more to themselves than what they earned in the real effort task compared to alloca-

[8]Details of how participants were matched and how earnings were determined are in the instructions in Appendix C.1.

tions when deciding as impartial spectators.

The first hypothesis is based on results from previous studies like Cherry et al. (2002) showing that participants tend to give more to themselves than what they earned, especially if such decisions have personal pay-off consequences and are made anonymously and privately. In our experiment, the allocation made as partial stakeholder is also made privately and anonymously, giving our participants more leeway to behave more selfishly. In contrast, the allocation made as an impartial spectator the absence of consequences on the deciders' payoff can serve as a cue for participants to view the situation in what Rustichini and Villeval (2014) calls as an "abstract moral setting" and can thus prompt them to follow more closely the proportionality principle.

Hypothesis 2: When deciding as partial stakeholders, deviations from the proportionality principle in the PI treatment will at least be equal to deviations in the IP treatment.

A strict adherence to the proportionality principle suggests that one allocates equal to the contribution in the real effort task. From the first hypothesis, we predict that deviations from this principle will be greater when deciding as a partial stakeholder. In the second hypothesis, we take into account possible history dependence of moral behavior which can magnify or lessen deviations from the proportionality principle in the allocation as partial stakeholders. This prediction is also based on findings documented in psychology. DeJong (1979) identifies the "foot-in-the-door" effect which shows how an initial small request accepted raises the probability of accepting costlier ones later on. However, participants can also display moral credentialing or balancing, acting as if an initial good behavior becomes a license to misbehave later on as Monin and Miller (2001) show.

In the IP treatment, participants can easily build a self-image that follow the proportionality principle since this does not affect their payoffs. However, this ties their decision as partial stakeholder in the next decision task as they cannot easily disregard the image they have built for themselves as an impartial spectator. Although deciding partially gives them room to behave more selfishly, this can be countered by the desire to maintain the positive self-image they have built previously. This may result in lower deviations from the proportionality principle as a partial stakeholder. Conversely, they can also use their adherence to the proportionality principle as an impartial

spectator as a license to behave more selfishly as a partial stakeholder.

In the PI treatment, deciding partially first allows participant to behave more selfishly in the absence of a previous task to condition one's present allocation. In a surprise subsequent task wherein they decide as an impartial spectator, their previous allocation as a partial stakeholder may serve as a guide to what is fair in efforts to convince themselves that their past and presumably selfish action also constitutes fair behavior. Thus, they can impose an allocation as impartial spectators that favors one party over another that deviates from the proportionality principle. However, they can also view the impartial allocation task as a different scenario than the partial allocation task, in which case the impartial decisions will not reflect the previous partial allocation.

Hypothesis 3: When deciding as partial stakeholders, deviations from the proportionality principle in the Exp treatment will be equal or greater than deviations in the Inexp treatment.

Hypothesis 3a: *The order of allocation tasks will have the greatest impact on the allocation decisions of inexperienced participants.*

This hypothesis is based on several studies, notably Matthey and Regner (2013), showing that experience in the laboratory makes participants more attuned to relevant factors in the experiment (e.g. their payoffs) and less towards peripheral factors. First, we expect more selfish allocations made among experienced participants compared to the inexperienced ones. On this basis, we expect experienced participants to adhere less to the proportionality principle to maximize their payoff.[9] Second, we also predict that the order manipulation in the experiment will affect the allocation decisions of inexperienced participants more than the allocation decisions of experienced participants.

4.4. Results

The experiment began with participants performing a real effort task. Participants on average correctly solved 34.5 tables (SD = 7.62), with experienced participants on average solving 4 tables more than inexperienced participants.

[9]The role of participants' experience in the laboratory is not yet well-explored in economic experiments. In a public good game,Conte et al. (2014) find that experienced participants on average both contribute less and expect others to contribute less.

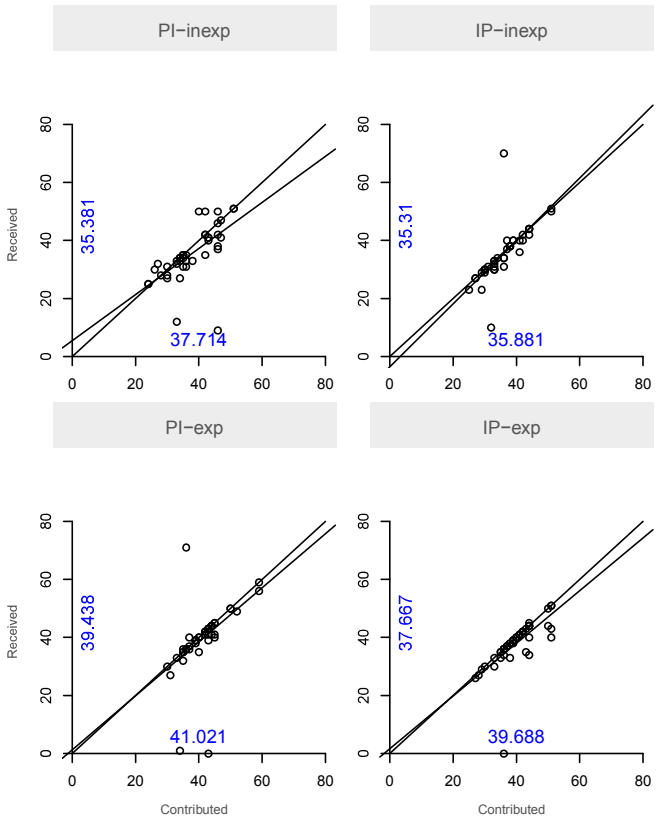
In this section, we first analyze decisions as an impartial spectator and proceed with testing our hypotheses related to decisions as partial stakeholder. We find some evidence in support of Hypotheses 1 showing more selfish allocations as partial stakeholder, and Hypotheses 3 indicating the effect of prior experience in allocation experiments. We find little support for Hypothesis 2 on the sole effect of the order of allocation task.

4.4.1. Allocation decisions as impartial spectators

We first look at allocation decisions in the impartial spectator task. We expect that in this task, where there is no conflict between self-interest and the proportionality principle, for impartial spectators to allocate resources that reflect each member's contribution.

Figure 4.1 displays the relation between the amount allocated to the better performer in the pair by the impartial spectator and the amount contributed by the better performer in the group in the PI (left panels) and IP (right panels) treatments and for the experienced (bottom panels) and inexperienced participants (top panels).[10] When these two amounts coincide, then the proportionality principle is perfectly applied. In the figure this case is represented by the points on the 45 degree line. Average amounts contributed and received are reported along the x- and y-axis respectively.

[10]When the performances in the real effort task is equal, we randomly choose one participant as the better performer in the pair.



Note: The points represent the amount received from the impartial spectator and the amount contributed in the real effort task by the better performer, by order of allocation task and prior experience. The solid 45 degree line represent strict adherence to the proportionality principle while the dashed line is a linear fitting of the data. Average amounts contributed and received are reported along the x- and y-axis respectively.

Figure 4.1.: Allocations to better performer by impartial spectator

Allocations to the better performer are quite close to the exact proportionality line even though they tend to be slightly below the line, regardless of the absolute level of the performance. This is also confirmed by a linear fitting of the data represented by the dashed line. We observe no substantial difference across levels of experience and order of undertaking the tasks. Overall, impartial spectators in both treatments tend to decide so as to preserve proportionality.

Consider now Panel A in Table 4.3 which shows the variation from a strict

adherence to the proportionality principle in the I-task. We compute extra rewards or ER_j^I for each impartial spectator through the following formula:

$$ER_j^I = C_j^I - E_j^I \tag{4.1}$$

where C_j^I is the percent of the total earnings of the pair allocated to one of the participants j, and E_j^I is the percent of the earnings in the real effort task that came from j. To illustrate, suppose participant j earned 75 ECUs in the real effort task and is paired with participant k who earned 25 ECUs in the real effort task. This means he earned 75% of the total pie to be divided by the impartial spectator ($E_j^I = 75\%$). If an impartial spectator deciding for this pair jk strictly follows the proportionality principle, then she will give 75% of the pie to participant j ($C_j^I = 75\%$) and ER_j^I thus takes the value of 0. If the impartial spectator does not strictly follow the proportionality principle (e.g. $C_j^I = 50\%$), then ER_j^I is thus -25%. ER_j^I can thus take either positive or negative values when an impartial spectator does not strictly adhere to the proportionality principle.[11]

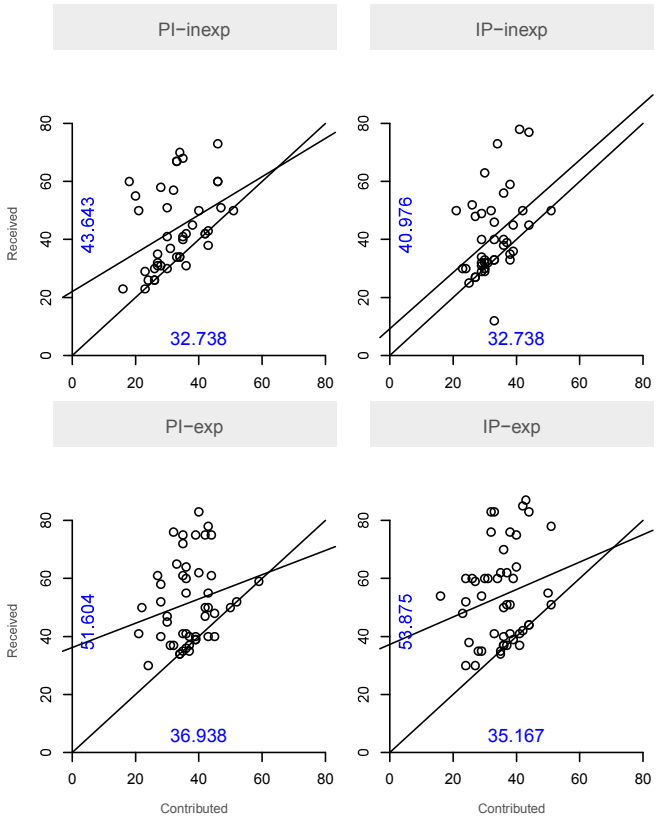
The mean values of extra rewards reported in Panel A are very close to zero for each treatment suggesting that in the I-allocation task the impartial spectator is likely to allocate earnings of the pair in a way which closely reflects each participant's contribution to the total earnings. This is similar to what we find in Figure 4.1 which indicate that the differences in slopes and intercepts between the strict proportionality line and linear fitting of impartial stakeholder data are close to zero. While we do not observe statistically significant differences when comparing IP and PI treatments (MW test: $z = 0.304$, $p = 0.713$), we observe that extra rewards given by experienced impartial stakeholders in both IP and PI treatments is statistically significantly different from extra rewards given by inexperienced impartial stakeholders (MW test: $z = 1.653$, $p = 0.098$). This gives us our first piece of evidence in support of Hypothesis 3.

4.4.2. Allocation decisions as partial stakeholders

In this section we focus on the decision of partial stakeholders. Figure 4.2 provides a representation of the allocation choices in each treatment. The graph illustrates the relation between amount contributed by the partial stakeholder and amount allocated to herself (regardless of being the better

[11] To simplify computation, j is consistently the the better performing participant in the pair.

performer in the group), in treatment PI and IP respectively. In the figure, choices of the experienced (Exp) and inexperienced (Inexp) are kept separate. The solid line identifies allocations that match exact proportionality between contributions and amount allocated to oneself. Average amounts contributed and received are reported along the x- and y-axis, respectively.



Note: The points represent the amount allocated to oneself by the partial stakeholder and the amount contributed in the real effort task, by order of allocation task and prior experience. The solid 45 degree line represent strict adherence to the proportionality principle while the dashed line is a linear fitting of the data. Average amounts contributed and received are reported along the x- and y-axis respectively.

Figure 4.2.: Allocations by partial stakeholder

Figure 4.2 shows that decisions of the partial stakeholders largely tend to favor themselves over their counterpart. In both order of tasks and both

levels of experience, most of the observations are well above the equity line, with participants allocating more to themselves than what they contributed to the group. This is confirmed by a linear fitting of the data (dashed line) showing that on average partial stakeholders claim more resources for themselves than what they contributed in the real effort task as was predicted in the first hypothesis. Inexperienced participants on average adhere more to the proportionality principle than experienced participants, as shown also by average values.

We now look at Panel B in Table 4.3 which refers to the P-allocation task and it indicates the percent of extra rewards for partial stakeholders ER_i^P. We computed ER_i^P for each partial stakeholder as:

$$ER_i^P = C_i^P - E_i^P \tag{4.2}$$

where C_i^P is the percent of the total earnings allocated to himself and E_i^P is the percent of the earnings in the real effort task that came from him. To illustrate, if a participant i earned 60 ECUs in the real effort task and is paired with a participant h who earned 40 ECUs in the real effort task . He therefore earned 60% of the total pie to be divided by himself as a partial stakeholder (i.e. $E_i^P = 60\%$). If he decides to give himself 60 ECUs (i.e. $C_P^i = 60\%$), then $ER_i^P = 0$. This constitutes strict adherence to the proportionality principle. When the proportionality principle deviates in favor of the partial stakeholder (or in favor of the other subject in the pair), it takes positive (negative) values.

Panel B reveals that the averages of ER_i^P are well above zero in all treatments, suggesting that partial stakeholders on average tend to assign to themselves more than what they are entitled to. This also confirms what we find in Figure 4.2 which shows that the intercept of the linear fitting of partial stakeholder data is higher than the intercept of the strict proportionality line in all panels. The slopes of the linear fitting of the partial stakeholder data are also flatter than the strict proportionality line in all panels, except in the IP-inexp which is almost parallel to the 45-degree line.

We conduct Mann-Whitney tests to see if the differences indicated in the figure on partial stakeholder allocations are statistically significant. Although we find no significant difference between the IP and PI treatments (MW test: z = 0.326, p = 0.745), we confirm that inexperienced partial stakeholders assign less extra rewards to themselves than experienced partial stakeholders (MW: z = 2.597, p = 0.009), especially in the IP treatment. This is again another finding that is in line with the third hypothesis.

Table 4.3.: Performance in the real effort task and extra rewards in allocation tasks

Panel A: ER_I^j in Impartial Allocation Task				
	PI	IP	PI and IP	MW Test PI vs. IP
Inexp	0.83	0.42	0.63	z=0.097
	(11.39)	(9.94)	(10.63)	p=0.923
Exp	-1.16	0.60	-0.28	z=0.735
	(12.97)	(8.63)	(11.12)	p=0.462
Inexp & Exp	-0.23	0.52	0.14	z=0.304
	(12.23)	(9.37)	(10.87)	p=0.713
MW Test Inexp vs. Exp	z=0.793	z=1.745	z=1.653	
	p=0.428	p=0.081	p=0.098	
Panel B: ER_i^p in Partial Allocation Task				
	PI	IP	PI and IP	MW Test PI vs. IP
Inexp	16.23	12.38	14.311	z=0.997
	(19.82)	(19.33)	(19.56)	p=0.319
Exp	20.44	27.04	23.74	z=1.386
	(20.33)	(23.01)	(21.85)	p=0.166
Inexp & Exp	18.48	20.20	19.34	z=0.326
	(20.09)	(22.49)	(21.28)	p=0.745
MW Test Inexp vs. Exp	z=0.817	z=2.839	z=2.547	
	p=0.414	p=0.005	p=0.009	

Note: Standard deviations are in parentheses. The table reports Mann-Whitney two sample test on the two treatment variations: order of allocation tasks (row values) and prior experience in allocation experiments (column values). ER stands for extra rewards and takes the value of zero if the proportionality principle is strictly followed and deviates from zero if the proportionality principle is not strictly followed.

4.4.3. Treatment effects on allocation decisions as partial stakeholder

Our analysis thus far has looked at decisions as an impartial spectator and partial stakeholder separately.

In this part, we investigate our main hypotheses and focus on extra rewards as partial stakeholder. We begin with an ordinary least squares regression to deepen our understanding of the allocation decisions made as partial stakeholder and the effects of our treatments in the form of.

$$ER_i^p = X_i^T \beta + \varepsilon_i \tag{4.3}$$

The dependent variable is given by extra rewards as partial stakeholders

ER_{P_i}. As main explanatory variables of X_i we include :the treatment dummy variable *IP* which is equal to 1 when participants first face the impartial spectator task and then the partial stakeholder task and being equal to 0 when the order of the tasks is reversed and a dummy variable *Inexp* which is 1 if the participant had not participated in other allocation experiments before and 0 otherwise. We also include an interaction variable *IP*Inexp* to test the joint effect of the order of allocation tasks and prior experience in the third hypothesis.

We control for performance in the real effort task following the work of Gill and Stone (2015) showing that effort choices and relative performance can also influence distributional concerns. We thus add in the regression the variable *OwnPerform* which measures the level of own performance in the real effort task and *DiffPerform* which measures the difference in a participant's own performance and her partner.

We also control for the possibility that participants think other fairness principles are the social norm through the variables *Proportionality* and *Equality.* These variables are based on the participants' answers in the guessing task at the end of the experiment. The *Proportionality* variable takes the value of 1 if a participant deems that a strict proportionality is the social norm. Answers that rated as "socially very acceptable" or "socially quite acceptable" the option of dividing the pie 40 and 60 between two people who contribute 40 and 60 respectively take the value of 1 and take a value of 0 otherwise. We also have the variable *Equality* which takes the value of 1 if a participant said that dividing the pie 50 and 50 between two people who contributed 40 and 60 respectively is "socially very acceptable" or "socially quite acceptable;" other responses take the value of 0. The general pattern of social appropriateness ratings shows that the vast majority of participants deems the allocation which exactly matches proportionality of inputs and rewards (i.e. 40—60) and the equal split allocation (i.e. 50—50) as socially acceptable.[12]

Table 4.4 presents the results of the regression.

[12]The complete breakdown of participants' ratings can be found in Table C.1 in Appendix C.

Table 4.4.: Ordinary least squares regression on extra rewards as partial stakeholders

	Model 1		Model 2		Model 3	
Constant	32.209***	(9.718)	28.232***	(10.010)	32.263***	(11.629)
IP	1.486	(2.930)	6.233	(4.165)	6.210	(4.189)
Inexp	-10.279***	(3.941)	-5.091	(4.084)	-4.963	(4.106)
*IP*Inexp*			-10.083*	(5.804)	-10.269*	(5.780)
Real effort task						
OwnPerform	-0.255	(0.258)	-0.211	(0.258)	-0.218	(0.254)
DiffPerform	-0.552***	(0.174)	-0.574***	(0.171)	-0.550***	(0.174)
Social norm vignette responses						
Proportionality					-8.889	(6.065)
Equality					-0.469	(5.163)
R^2	0.177		0.191		0.201	
F (p-value)	<0.001		<0.001		<0.001	
N	180		180		180	

Note: ***p<0.01, **p<0.05, *p<0.10. Standard errors are reported in parentheses.

As the constant of all 3 models in Table 4.4 shows, partial stakeholders take for themselves considerable positive extra rewards. This remains significant even when we control for belief in proportionality or equality as the social norm in the allocation task. The size of extra rewards is significantly lower among inexperienced than among experienced participants in Model 1 although there is no significant difference in extra rewards depending on the order of undertaking allocation tasks in all 3 models. This is consistent with what we have discovered previously in the two sample Mann-Whitney tests.

Regression outcomes of Models 2 and 3 shed some more light on the source of the difference among experienced and inexperienced participants. As the interaction term *IPxInexp* shows, inexperienced participants react to the order of the allocation tasks. Specifically, inexperienced participants who are first exposed to the I-allocation task claim less extra rewards for themselves than those exposed to the P-allocation task first. This result holds even when controlling for own performance and differences in performance and belief in proportionality and equality as a social norm for the allocation task. We thus find some support for Hypothesis 3a on the joint effect of order of allocation tasks and prior experience.

4.4.4. Discussion

Our results support Hypothesis 1, suggesting that when their personal interest is not affected by the allocation, participants are likely to adhere to an idea of fairness based on the proportionality principle. In contrast, when deciding as partial stakeholders, they are more likely to deviate from proportionality to their advantage. We find no clear evidence supporting Hypothesis 2 and showing the sole effect of the order in which the I- and P-allocation tasks are presented. The extra rewards allocated as partial stakeholder do not differ significantly depending on the order of allocation task.

However, Hypothesis 3 is supported in our data: experienced and inexperienced participants allocate differently as partial stakeholders. The order in which participants undertake the two allocation tasks affects the decisions of inexperienced participants but not that of experienced participants. That is, inexperienced participants first exposed to the impartial allocation task claim less extra rewards for themselves than those first exposed to the partial allocation task. On the other hand, we observe no difference between the decisions of experienced participants who were in the PI treatment and experienced participants in the IP treatment.

Our findings relate to several studies exploring moral hypocrisy and self-image. Like Barden et al. (2005), we find evidence suggesting that the adage "saying one thing and doing another" applies for a subset of participants who are inexperienced in allocation experiments. Barden et al. (2005) test in a series of experiments how reversing the order of giving a public statement and performing a private behavior changes how a participant perceives someone else as hypocritical in areas such as safe sex and healthy living. While our study also uses a similar order manipulation, we focus on possible changes in personal decisions and not in the perceptions participants have of other people. Rustichini and Villeval (2014) also show through a dictator game experiment that individuals try to strike a compromise between self-image and payoffs by adjusting their decisions when they play the game for real money and when they play the game where their choices have no strategic consequences.

The results of the experiment can also be interpreted in light of what we discover in the vignette study to elicit social norms about the proportionality principle. More participants favored a 50—50 division than an allocation that exactly matched proportionality of inputs and rewards. It appears that a 50—50 division in the dictator game is the perceived social norm among our participants. However, even though this might be the perceived social

norm, participants in our experiment rarely split the pie in the partial allocation task this way. This is similar to what Krupka and Weber (2013) find in their test of their elicitation technique. They further state that norm ratings alone do not simply track behavior independently, suggesting that people can be aware of a different fairness principle as a social norm from what they apply to themselves.

4.5. Conclusion

In this paper we conducted an experiment to see whether and to what extent people's allocation decisions reflect their fairness ideals according to the proportionality principle. We manipulated the order in which they undertake two allocation tasks and the previous laboratory experience of participants in allocation experiments. Participants first generate resources in a real effort task and then distribute them. The partial allocation task proceeds like a standard dictator game wherein participants determine the earnings for themselves and another participant. In the impartial allocation task, the participant determines the earnings for two other participants.

Our first finding is in line with what has been previously discovered by Konow (2000): participants allocate more to themselves than what they have earned when choices have direct payoff consequences for themselves. Our second finding is that the sequence of roles as decision maker in the dictator game has an effect for participants who have not yet participated in an allocation experiment in the laboratory before. Specifically, inexperienced participants are more likely to follow the proportionality principle as partial stakeholders when deciding impartially first.

Why should the sequence of partial and impartial roles in an allocation task matter? One conjecture is that people would like to have a rather fair, even generous, image of themselves. Deciding impartially first, wherein allocations entail no personal material cost, allows people to reveal what they think is the ideal allocation between two parties. When their allocation can have personal consequences, as in the partial stakeholder allocation task, people's self-interest can run against their fairness principles. In line with the idea of history-dependence in moral behavior by Bénabou and Tirole (2011), the decision of the previous self (i.e., the decision in the first task) becomes a form of self-signal to the future self (i.e., how to decide in the second task).

However, we observe that order of decision matters only among partici-

pants who have not participated in allocation experiments before, suggesting that factors as laboratory experience deserve further attention in the dictator game literature. Although dictator game giving has been considerably studied already, these experiments use findings from some individuals who have participated in experiments before. Our study shows that there may be a need to take into consideration the role of participants' experience in previous experiments when analyzing dictator game giving in particular and laboratory behavior in general.

We presented here an experiment to check how reversing the order in which participants undertake two allocation tasks that differ in their personal stake in the final outcome can alter their fairness principles. While using the proportionality principle and impartial spectator theory affords us with an experimentally verifiable measure of fairness in relation to self-image concerns, it also limits the definitions of fairness used by people that we can test. As hinted by the vignette study, people are aware of alternative definitions of fairness. If this study looks into how malleable people's fairness ideals are according to the proportionality principle, future questions can also explore how people adjust their actions according to other definitions of fairness and vice-versa.

5. Conclusion

> *"If there's no meaning in it," said the King, "that saves a world of trouble, you know, as we needn't try to find any. And yet I don't know," he went on [...]; "I seem to see some meaning in them, after all."* (p. 106)
>
> *(Alice's Adventures in Wonderland, 1865, p. 106)*

The dissertation began with the presentation of two theories of identity, one by George Akerlof and Rachel Kranton (2000) and another by Roland Benabou and Jean Tirole (2011). The specific goal of the dissertation was to test some of their behavioral assumptions and predictions as a way of locating the effects of identity and institutions at the level of individual choice and behavior. I designed and conducted a series of laboratory experiments that explored the following economic situations: exchange relations under formal and informal rules; hiring in a mixed gender labor market; and the allocation of (real-effort) earnings. I chose laboratory experiments as my methodological tool because it allowed me to capture the core complexities of the research problems posed and made establishing possible links and mechanisms at the level of individual decisions simpler.

Each chapter ended with its own conclusion about the specific theories and research questions being explored. In this final chapter, I broaden the scope and review the experimental results in light of the identity theories presented in the introduction. I then discuss the general implications of the results and end by presenting the pathways for future research.

5.1. Review of experimental findings

Akerlof and Kranton's theory begins with a model of identity based on social difference. Each person has a mapping of her own category and other people's categories and with each category comes a set of prescriptions that tells how a person belonging to the category should behave. A person's

identity depends on her assigned social category, to what extent her own given characteristic matches the ideals of her assigned category, and to what extent her actions and other's actions follow their respective categories's prescriptions. A person's utility thus depends on a their actions, the actions of others, and on their identity.

I test key predictions of Akerlof and Kranton's analysis in Chapter 2 and 3, namely, that people have identity-based payoffs from their own actions; that people have identity-based payoffs derived from others' actions; and that third parties can generate (persistent) changes in these payoffs. I test these predictions in two experiments wherein a participant is part of one category or group (in-group), but not part of another (out-group).

In Chapter 2, I disentangle the possible effects of formal and informal rules on behavior in exchange relations. I find that indeed, third parties can generate changes in identity-based payoffs—formal rules affect the decision of two parties to trust and perform in an exchange relation. The effect of third party enforcement trumps the impact of common knowledge of shared group identity in encouraging cooperative behavior of two parties. Identity-based payoffs matter only in the decision to perform a contract. I represent this exchange relation through a contract game wherein two players decide sequentially to enter an agreement, and depending on their decisions (trust or not trust for first player, breach or perform for second player), produce joint surplus. An exogenous chance move can find the second mover liable if he breaches the agreement. This chance move represents the quality of contract enforcement or formal institutions. To represent informal rules, I induce group identity through minimal groups and create shared expectations through common knowledge of identity in some treatments.

Chapter 3 at the outset looks at theories of gender discrimination in hiring. Categories in Akerlof and Kranton's theory can come with status and differences in status and deviations from prescribed behavior can give rise to discrimination. In an experiment where I investigate whether the gender mix of employers influences who gets hired, I find that when a female employer is paired with a male employer, a female applicant is more likely to be hired, holding all else equal. Declines in numerical representation of females as employers can make salient in the laboratory the minority status of women in the workplace in real life and thus, female employer respond by hiring more female applicants.

While Akerlof and Kranton's theory is informed by research in social psychology, Benabou and Tirole's theory adopts ideas from cognitive psychology. People's identities are formed through a self-inference process wherein

they based their present action on past actions and the desirability of the self that the action can make them.

Chapter 4 investigates the assumption of history dependence in Benabou and Tirole's theory in the context of allocation decisions. I find two possible forms of history dependence in the final chapter: first, in terms of quence of allocation tasks: and second, in terms of previous participation in laboratory experiments. I find that participants who have not participated in allocation experiments before are more likely to be affected by the sequence of allocation tasks. More specifically, they were more likely to follow their stated fairness ideals when deciding partially if they were made to decide impartially first.

5.2. Policy implications

What implications do the results of the experiments in this dissertation have? First, it is important to note that the experiments on their own do not have direct policy lessons.[1] They are rather parts of a body of scientific work that explores theories on identity and institutions. It is from these theories, not from the experiment themselves, from which we can draw policy implications. That being said, implicit in the theories I have presented are the following points which bear explicit mention now: (1) institutions shape how we see the world; (2) institutions shape how we see our selves. Given these two points, introducing reform may have important unintended consequences that may be difficult to foresee not just on the aggregate level, but also on the individual level. Friedrich Hayek's (1948, p.14) caution about "constitutional limitation of man's knowledge and interests, the fact that he cannot know more than a tiny part of the whole of society and that therefore all that can enter into his motives are the immediate effects which his actions will have in the sphere he knows" is made all the more relevant here.

Policymakers affect how institutions are designed and implemented. Institutions are not just rules of the game, but as Mary Douglas (1996) writes, ways of seeing the world. Institutions can expand our circles of interests and shape incentives such that socially beneficial outcomes are more likely to happen. Each study in this dissertation demonstrates how an exogenous change in formal rules can change outcomes.

[1] Zeiler (2010) also makes a similar point and details the precautions one must consider when interpreting experimental results.

Institutions shape how we see our selves. It can distort, magnify, or obscure what we see in the looking glass through which we judge ourselves and others. It can blur or highlight the lines between categories and transform the prescriptions that come with them. The World Development Report (2015) emphasizes how an approach that takes into account can inform the policies we design. The report gives the example of the persistent exclusion of some groups in society even when formal barriers to participation and access are removed. Because categories and belief systems are inherited from previous generations, the effects of abolished formal institutions can persist. This consideration is overlooked in the standard approach in economics that suggests that groups are excluded because they lack access to markets and resources or are under exploitative structures.

Institutional reforms can redefine identities but also shift individual preferences that have larger implications for the economy. For example, i Font and Cowell (2015) claim that the rescaling of European identity arising from the introduction of a common currency increased preferences for redistribution and reduced the importance of national pride.

This dissertation argues why it is worthwhile for both researchers and policymakers to take an approach that considers the interaction of identity and institutions in exploring a number of economic puzzles. There still remain many questions that could benefit from this approach. Understanding the political economy of institutional reform, especially in instances when identities are implicated in decisions concerning reform, remain relevant. It also remains meaningful for both researchers and policymakers to better understand the persistence of weak, formal institutions, or of how (non-)malleable identities are to different frames and varying incentives using the approach presented in this dissertation.

A. Appendix to Chapter 2

A.1. Instructions

The instructions were originally written in English and were translated in German for the experiments. They were then back-translated to English to verify accuracy.

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please switch off your mobile and remain quiet. It is strictly forbidden to talk to the other participants. Whenever you have a question, please raise your hand and one of the experimenters will come to your aid.

You will receive 2.50 Euros for showing up on time. Besides this, you can receive more. The show-up fee and any additional amounts of money you may receive will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., the others will not see your earnings.

During the experiment we shall speak of ECUs (Experimental Currency Unit) rather than Euros. The conversion rate between them is 1 ECU = 0,05 Euros. This means that for each ECU you get you will be paid 0,05 Euros.

Before the experiment started everyone drew a bracelet that is either Yellow or Red. There are an equal number of Yellow or Red bracelets in the bag. You have been assigned to the Yellow group if you received a Yellow bracelet and the Red group if you received a Red bracelet. Please wear your bracelet throughout the experiment.

What is the decision you have to make?

At the beginning of the experiment, you will be assigned the role of Person 1 or Person 2. The number of participants assigned to the role of Person 1 will be equal to the number of participants assigned to the role of Person 2. Each person's task is to choose between two options. All decisions made will not be known by the other participants.

Person 1

Below are the possible contents of the screen that you will encounter if you are Person 1. It tells you how many ECUs you earn depending on what you choose and what the other person chooses.

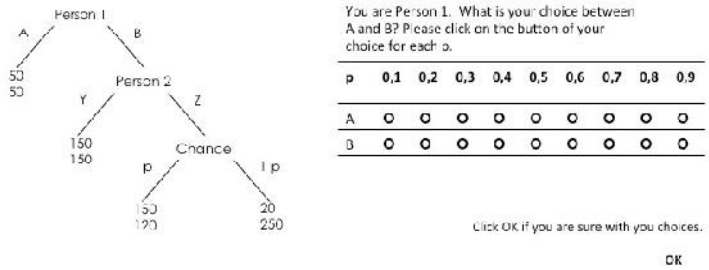


Figure A.1.: Decision screen for Person 1

The diagram on the left reads as follows. If you are Person 1, you have to choose between A and B. If Person 1 chooses A, Person 1and Person 2 receive 50 ECUs each. If Person 1 chooses B, Person 2 gets to choose between Y and Z. If Person 2 chooses Y, Person 1 and Person 2 receive 150 ECUs each. If Person 2 chooses Z, chance decides about your earnings. Person 1 can earn 150 ECUs and 20 ECUs with a given probability (p) that change in increments of 0,10. For example:

A p of 0,1 means you have a 1 out of 10 chance of receiving 150 ECUs and a 9 out of 10 chance of receiving 20 ECUs if you choose B AND Person 2 chooses Z.

A p of 0,2 means you have a 2 out of 10 chance of receiving 150 ECUs and an 8 out of 10 chance of receiving 20 ECUs if you choose B AND Person 2 chooses Z...

...and so on.

The task is to choose between A and B for each given probability p. That means you must make at least nine decisions. You make this decision by clicking on the buttons corresponding to your choice on the tables you see at the right side of the screen.

Person 2

If you are person 2, below is a sample of the contents of the screen you will encounter. It has the same contents as the screen for participants assigned the role of Person 1 but the choice to be made now is between Y and Z. It tells you how many ECUs you earn depending on what you choose and what the other person chooses.

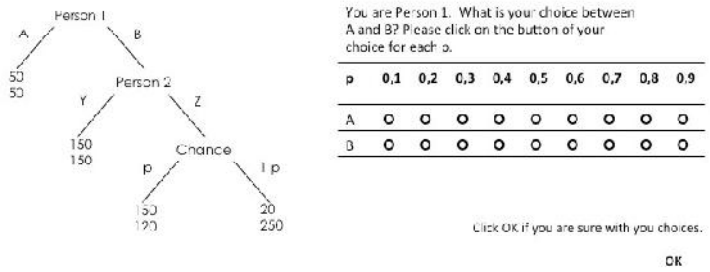


Figure A.2.: Decision screen for Person 2

The task is to choose between Y and Z. If Person 2 chooses Y, Person 2 and Person 1 receive 150 ECUs each. If Person 2 chooses Z, chance decides about your earnings. Person 2 can earn 120 ECUs and 250 ECUs with a given probability (p) that change in increments of 0,10. For example:

A p of 0,1 means you have a 1 out of 10 chance of receiving 120 ECUs and a 9 out of 10 chance of receiving 250 ECUs if Person 1 chooses A AND Person 2 chooses Z.

A p of 0,2 means you have a 2 out of 10 chance of receiving 120 ECUs and an 8 out of 10 chance of receiving 250 ECUs if Person 1 chooses A AND Person 2 chooses Z.

...and so on.

The task is to choose between Y and Z for each given probability p. That means you must make at least nine decisions You make this decision by clicking on the buttons corresponding to your choice on the tables you see at the right side of the screen.

How will you be paid?

At the end of the experiment, you will be randomly paired with a Person

2 if you are Person 1 and a Person 1 if you are Person 2. Then one of the probability conditions will be randomly chosen and your decision there will be used to determine your final payment.

Kindly think carefully of each of your choices because each has an equal chance of being selected for your final payment.

At the end of the experiment, you will be asked to answer some questions. All your answers will be kept private and will not be used to identify you.

Please click OK on the screen if you are finished reading the instructions.

Post-experimental questions

This part is displayed on screen. Participants rated the following items according to the following otions: strongly agree, agree, neither agree or disagree, disagree, or strongly disagree with them.

1. Most people tell a lie when they can benefit by doing so.

2. Those devoted to unselfish causes are often exploited by others.

3. Some people do not cooperate because they pursue only their own short-term self- interest. Thus, things that can be done well if people cooperate often fail because of these people.

4. Most people are basically honest.

5. There will be more people who will not work if the social security system is developed further.

6. Generally speaking, people can be trusted and you need not be too careful when dealing with others

A.2. Post-experimental question regression

The answers to the questions were coded such that “strongly agree” takes the value of 1, “agree” is 2, “neither agree or disagree” is 3, “disagree” is 4, and “strongly disagree” takes the value of 5.

Table A.1.: Logistic regression estimates on trust and perform

	Trust		Performance	
	β	z	β	z
Contract enforcement probability (baseline is p=0.1)				
0.2	0.1890	(0.90)	0.6478**	(2.51)
0.3	1.3700***	(3.66)	0.4177	(1.52)
0.4	2.4717***	(4.95)	1.2144***	(3.05)
0.5	4.0080***	(5.11)	1.4822***	(2.89)
0.6	4.9135***	(5.27)	2.4098***	(3.92)
0.7	4.7325***	(5.08)	2.7651***	(4.23)
0.8	4.7325***	(2.42)	3.2225***	(4.79)
0.9	4.0080***	(4.38)	3.2217***	(4.49)
Group identity (baseline is no group)				
In-group	0.2980	(0.36)	1.0687**	(1.91)
Out-group	0.1413	(0.18)	0.0772	(0.13)
Participant variables				
Female	-1.3896**	(-1.89)	0.7073	(1.48)
German	-0.9796	(-0.47)	-0.2975	(-0.41)
Age	0.0423	(0.90)	0.0337	(0.54)
Questionnaire				
People tell a lie when they can benefit	0.3397	(0.90)	-.1188	(0.41)
Unselfish people are often exploited by others...	-0.0485	(-0.16)	0.0099	(0.04)
People are self-interested...	0.0957	(0.21)	0.3702*	(1.83)
Most people are honest	0.1089	(0.24)	0.1940	(0.63)
On social security system	-0.2592	(-0.94)	-0.5281	(-2.79)
People can be trusted...	-0.5003	(-1.15)	-0.1117	(-0.44)
Constant	-0.1779	(-0.04)	-2.8310	(-0.96)
Log-Likelihood	130.08***		83.61***	
Wald χ^2 (19)	45.05***		39.75***	
N of observations	810		810	
N of groups	90		90	

Note: $***$ (<1%), $**$ (<5%), and $*$, (<10%),mark which variables were statistically significant in each model and indicate their associated levels of significance. The dependent variable takes a value of 1 if participant chose to trust or perform, 0 if not. The reported z statistics in parentheses are based on bootstrapped standard errors (200 replications)

None of the answers to the questionaire items were significantly correlated with the choice to trust, although some of these answers were significantly correlated with the choice to perform. The stronger a participant disagrees with the statement that some people do not cooperate to pursue their own short-term self interest, the more likely he is to choose perform; similarly, the stronger a participant agrees with the statement that there will be more people who will not work if the social security system is developed further, the more likely he is to choose perform.

B. Appendix to Chapter 3

B.1. Instructions

Original instructions are in German. Below is a translation of instructions to English.

B.1.1. Instructions for PARTICIPANTS A

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please switch off your mobile and remain quiet. It is strictly forbidden to talk to the other participants. Whenever you have a question, please raise your hand and one of the experimenters will come to your aid.

You will receive 2.5 Euros for showing up on time. Besides this, you can earn more. The show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., the others will not see your earnings.

During the experiment we shall speak of ECUs (Experimental Currency Unit) rather than Euros. The conversion rate between them is 1 ECU = 0.1 Euros.

This means that for each ECU you earn you will receive 0.1 Euro.

At the beginning of the experiment, you were given a piece of paper with the password for your computer. Please type this password to activate your screen.

You will then be asked to give your level of education and age. Please be assured that the information you give will not be used to identify you and will be used for experimental purposes only.

In this experiment, there are two types of participants: Participant A and Participant B. You were randomly assigned the role of Participant A.

As Participant A, you will first be asked to convert letters into numbers. Your screen will display a table with two columns. The first column indicates letters and the second column indicates their corresponding numbers.

You will be shown a letter and you will have to enter the corresponding number in a box on your screen. You must validate your answer by pressing the 'OK' button.

The conversion table of letters and numbers is modified only when you have correctly entered the correct number. A new conversion table appears when you entered the correct answer.

Please see Figure B.1 for an example of the screen you will encounter in this task.

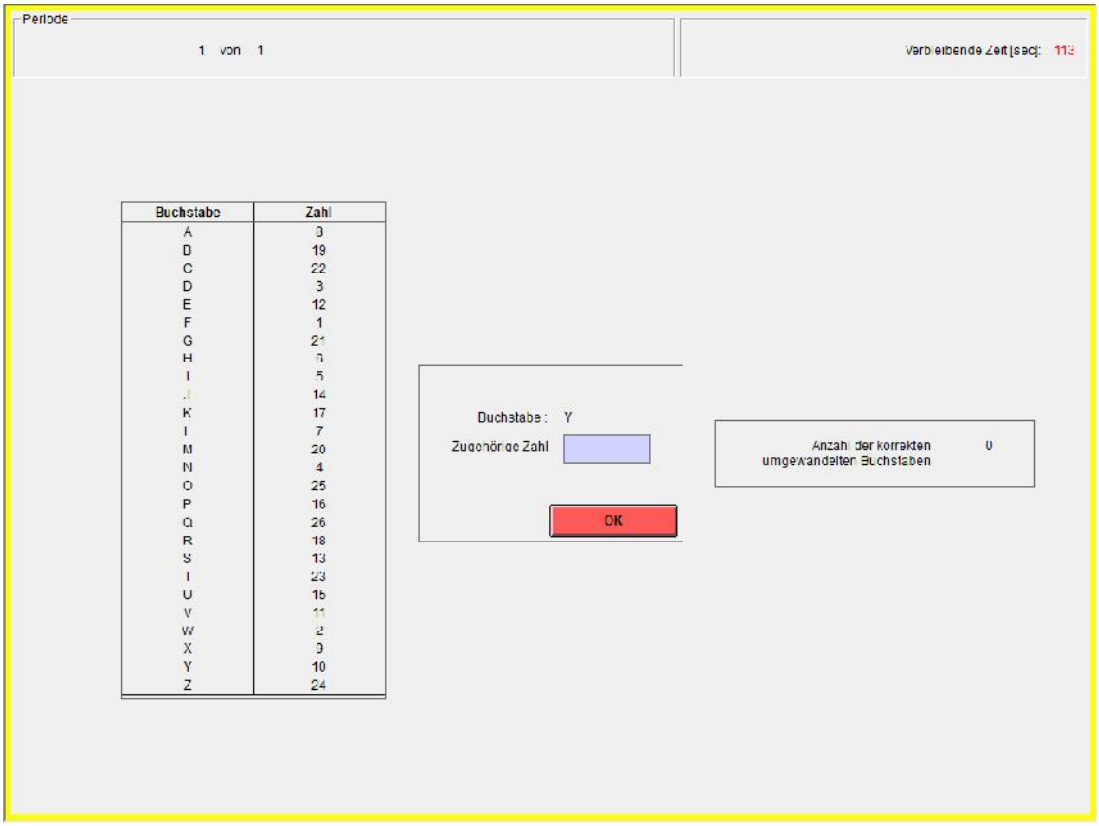


Figure B.1.: A screen shot of the conversion task

In this example, you must enter the number 10 and click "OK" for a new conversion table to appear. At the right hand of the screen is the number of correctly converted letters. At the upper right hand of the screen is the remaining time in seconds.

You will be given the opportunity to practice this task during two minutes to familiarize yourself with the task. The number of problems solved during

this practice period will not affect your earnings.

You will then go on to perform the task for 4 minutes. Depending on your performance in this round, you will be assigned to one of the following groups: "High", "Middle," and "Low." In the group "High" are one-third of Participants A who converted the most number of letters into numbers. In the group "Low" are one-third of Participants A who converted the least number of letters into numbers. All remaining participants A are then assigned to the group "Middle"

The points you earned in this first round of conversion is not relevant for your earnings.

After completing this task, participants who were assigned the role of Participant B for this experimental session will be shown your group based on your performance in the first round of conversion, your level of education, and your age.

Participants B will decide whether they want your performance in a second round of this tasks to determine their payoff or not.. If you are selected, then whatever you earn in the second round is also what participants B will earn. So if you earn XXX ECU in the next round then participants B will also earn XXX ECU. You will not be informed of whether you were selected or not.

Whether you are selected by a Participant B or not, you will be able to perform the conversion task once again for 4 minutes and you will be paid for each correct conversion. For each letter you correctly convert to a number, you will get 1 ECU. The more letters you convert correctly into a number, the more ECUs you will get. Your payment does not decrease if you give an incorrect answer to a problem, but you are not paid for incorrect answers.

Please click OK on the screen when you are done reading the instructions. You will then be asked to answer some questions to check your understanding of the instructions. The experiment will go on only once all participants answered all questions correctly.

B.1.2. Instructions for PARTICIPANTS B

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please switch off your mobile and remain quiet. It is strictly forbidden to talk to the other participants. Whenever you have a question, please raise your hand and one of the experimenters will come to your aid.

You will receive 2.5 Euros for showing up on time. Besides this, you can earn more. The show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., the others will not see your earnings.

During the experiment we shall speak of ECUs (Experimental Currency Unit) rather than Euros. The conversion rate between them is 1 ECU = 0.1 Euros.

This means that for each ECU you earn you will receive 0.1 Euro.

The experiment is composed of two parts. Only the first part is relevant for your payment. The instructions for the first part follow shortly. The instructions for the second part will be shown on your screen when you are finished with the first part.

Part 1

In this experiment, there are two types of players: Participant A and Participant B. You were randomly assigned the role of Participant B.

At the beginning of the experiment, you were paired with another Participant B to form a pair. You and your fellow Participant B in the pair will be shown the same pair of Participants A. Participants A have performed a task of converting letters into numbers based on a table of correspondence between letters and numbers. They had 2 minutes to practice this task. They then performed the same task for 4 minutes. Participants A have been assigned according to their performance in this 4-minute round in one of the following groups: High, Middle, and Low.

In the group "High" are one-third of Participants A who converted the most number of letters into numbers. In the group "Low" are one-third of Participants A who converted the least number of letters into numbers. All remaining participants A are then assigned to the group "Middle" Based on the performance of participants who already performed this task in a previous session, those assigned to the group low can convert around 40 to 60 letters, those in the group middle can convert around 61 to 80 letters, and those in the group high an convert more than 80 letters.

You and the other Participant B in your group will decide which Participant A will perform the conversion task again and determine your earnings.

The performance of the selected Participant A determines your earnings and the earnings of the other Participant B in your group and also the earnings of Participant A. The means, for each letter that Participant A correctly converts in the second round of the task, you and Participant A receive 1

ECU.

Your task is to decide which of Participant A in the pair will perform the conversion task for another round of 4 minutes for you. The other Participant B encounters the same pairs as you but you will make the decisions individually. This means you will not know of the decision of the other Participant B. Participants A are also not informed whether they are selected or not.

Below is a sample of the decision screen you will encounter.

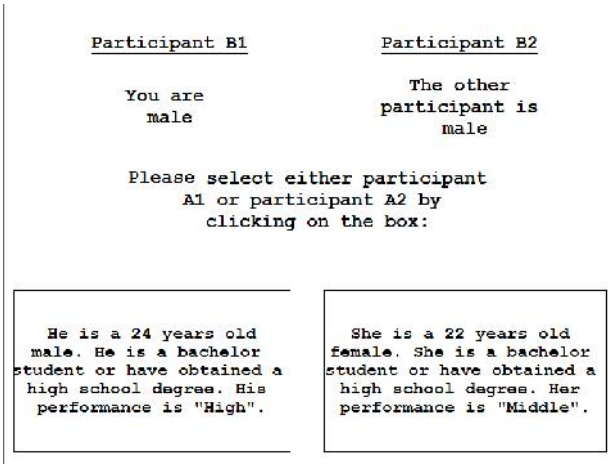


Figure B.2.: A sample of the decision screen for Participant B

In the screen shot above, you are Person B1 and the other participant in your group is Person B2. Your respective genders are also shown. Below this, you can see the two Participants A, Person A1 and Person A2. Apart from their performance in the first round of the conversion task, you will also be informed of each Participant A's age and education level. Please click on which Participant A you would like to determine your earnings in a second round of conversion task. When you have made your choice, another pair of Participants A will be shown. You will encounter each pair and each Participant A only once.

Overall, you will be shown 35 different pairs of Participants A to select from during the course of the experiment. Some of these participants are hypothetical, others are real.

After having made those 35 decisions, the computer will randomly select which of the pairs of real Participants A that you were shown will be relevant for your earnings. The same pair will also be relevant for your fellow

Player B. Because you do not know which pairs will be relevant for your pay-off, you should decide as if each pair was the one that will be chosen to be payoff relevant.

Then the computer chooses randomly whose decision in the relevant pair will be taken into account – yours or that of your fellow Player B. That is, with probability one-half the Participant A whom you chose will determine your earnings. Otherwise, it is the other Participant A who will determine your earnings. This means that your choice counts in only half of the cases. You will be informed at the end which pair of Participant A and which Participant A in the pair was chosen. Because your decision has the same chance to count as that of the other, please decide as if you are the one who will determine which Participant A is selected.

All Participants A will perform the conversion task again for 4 minutes. The Participant A who was selected according to the procedure above will NOT be told about your choice. Both you, your fellow player B and this player A will receive a payoff equal to this player's performance in this second round of the conversion task.

The Participant A who was not selected will also perform the conversion task again and receive a payoff equal to their performance, 1 ECU for each correctly converted letter. That performance does not determine your pay-off however, and you will not be told how other Participants A performed.

You will then be asked to answer some questions to check your understanding of the instructions. After answering the questions, your decision task will begin. To proceed, kindly click on the file "Participant B.ebs" that will appear on your screen. The folder where this file can be found is already open. Please wait a moment in case the file is not yet shown on your screen.

When the file appears on your screen, please double-click to open. Upon entering the laboratory, you were given a small sheet of paper with your participant number, cabin number, and password. Kindly enter this information on the screen when it is requested.

You will then be requested to enter your education level and age. Please be assured that the information you give will not be used to identify you and will be used for experimental purposes only.

Part 2: Implicit association test

We presented instructions for the IAT on the screen. Below is a sample of the screen participants encountered. In the middle of the screen is a word

that a participant sorts to one of the categories at the right and left side of the screen by pressing a right and left button respectively. If, for example, one has an implicit view of women being warm, then sorting the word women with female and warmth on the same of the screen will be faster than sorting it with words of female and competence on the same side of the screen. If one has an implicit view of men being competent, then sorting the word men with the words male and competence on the same side of the screen will be faster than sorting it with the words male and warmth on the same side of the screen.

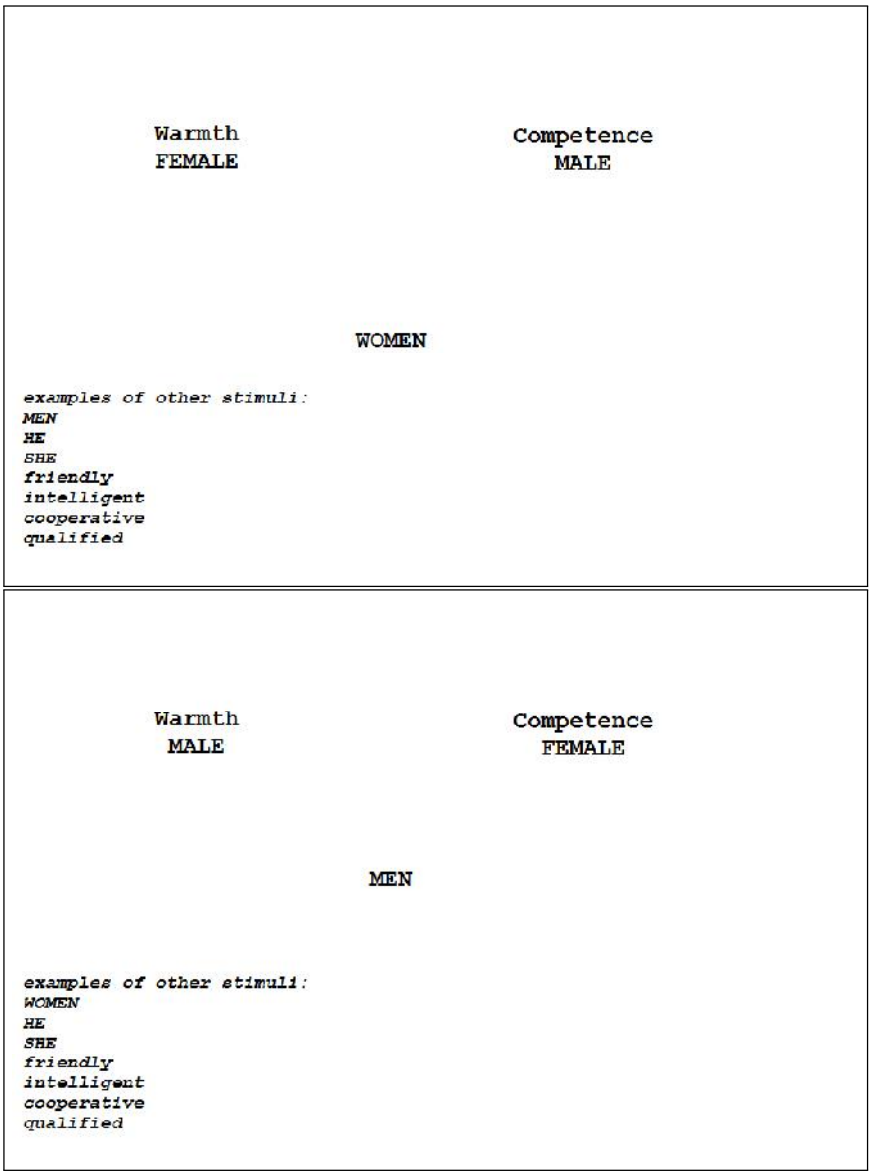


Figure B.3.: Screen shots of the implicit association test

B.2. Explicit attitudes

Below is the English translation of the post-experimental questionnaire to measure explicit attitudes.

1. Which factors did you take into consideration when you were making

your decision? (Gender / Age / Education / Combined Factors).

2. Did you believe that females perform better than males in Player A task? (Yes/ No).

3. Did you believe that males perform better than females in Player A task? (Yes/ No).

4. Did you deliberately choose female participant over male participant? (Yes/ No).

5. Did you deliberately choose male participant over female participant? (Yes/ No.)

6. Did you believe that younger people perform better than older people in Player A task? (Yes/ No).

7. Did you believe that higher educated people perform better than lower educated people in Player A task? (Yes/ No).

8. Would you have decided differently, if the gender of your fellow Player B had been different than what it was? (Yes/ No).

9. What was the gender of your fellow Player B? (Male/ Female).

10. Did you expect that your fellow Player B would select Player A with the same gender (as the fellow player B)? (Yes/ No). If yes, why?

11. Did you expect that your fellow Player B would select Player A with the opposite gender (as the fellow player B)? (Yes/ No). If yes, why?

12. Did you expect that your fellow Player B would take the gender of Player A into consideration? (Yes/ No). If yes, why?

13. Do you believe that there are areas in which the performance of females and males differ? (Yes/ No).

14. In the field of medicine? (Yes/ No). If yes, in which way do they differ?

15. In the field of law? (Yes/ No). If yes, in which way do they differ?

16. In the field of science? (Yes/ No). If yes, in which way do they differ?

17. In other fields? (Yes/ No). If yes, in which field and what are the differences?

18. What do you think was the purpose of the experiment?

19. How difficult did you find it to come up with a decision during the experiment? (7-point scale from "very easy" to "very difficult").

20. How understandable did you find the instructions of the experiments? (7-point scale from "very understandable" to "very difficult to understand").

21. Did you know any of the other participants of this experiment? (Yes/ No). If yes, how many people did you know?

22. Did you have any problems during the experiment? (Yes/ No). If yes, what are they?

23. Did you find the payment of this experiment appropriate? (Yes/ No).

24. Do you believe, that the experimenter will not misuse any of your personal data from this experiment? (Yes/ No).

25. What is your nationality? (German or _____).

26. Please indicate your current activities (e.g. studying, working)? _____ (maximum of 2 entries).

27. Are you currently a student? (Yes/ No). If yes, in what field are you currently studying?

28. Where did you live (most of your life)? (Big city with more than 1 million people/ Big city with more than 100.000 people/ City with more than 10.000 people/ A village/ Others: ____).

29. What are the main sources of your finances (e.g. family, scholarship, salary, government help/loan)?

30. How risk-taking are you in general? (Please give a number between 0 and 10. Zero for avoiding as much risk as possible and 10 for being very risk-loving).

31. Do you believe that two-people with the same qualifications should be paid equally, even though one person is more productive than the other? (Yes/ No/ Not Sure).

32. What do you think of the following statement? In general, people can be trusted. (7-point scale from "totally agree" to "do not agree at all").

33. What do you think of the following statement? Nowadays, we cannot trust on people so easily. (7-point scale from "totally agree" to "do not agree at all").

34. What do you think of the following statement? When you are dealing with stranger, it is better to be careful before you put your trust into that person. (7-point scale from "totally agree" to "do not agree at all").

35. Do you believe that most people will take advantage of you when there are opportunities? (7-point scale from "I very much believe" to "I do not believe at all").

36. Do you believe that most people treat others fairly? (7-point scale from "I very much believe" to "I do not believe at all").

37. Would you say that people most of the time strive to be helpful to others? (Yes/ No).

38. Would you say that people most of the time strive only to fulfill their own interest? (Yes/ No).

B.3. Applicants

In each session, 6 participants (3 males, 3 females) were assigned as Participant A or applicants. We ran 6 sessions giving us a total of 36 applicants. The average age of applicants in the experiment was 25.91 years (*SD 7.38*). Applicants who were studying for their Bachelor degree comprised 30.58% of the total, 52.78% of the applicants were studying for their Master degree, 5.56% of the applicants were doing their PhD degree, and 11.11% of the applicants listed "other" as their education level.

We test the assumption that there are no significant differences in the mean and variance of performance between male and female applicants.

B.1 shows how applicants performed by gender and the results of Mann-Whitney-Wilcoxon tests. We assume that the task is gender-neutral and have no strong *a priori* reason to believe why males or females should perform better than the other. If we compare performance between males and females in the first round, we find no statistically significant difference in performance. Once we compare performance between males and females in the second round which is paid, we find a statistically significant difference.

Table B.1.: Mean performance of Participants A by gender

	Male	Female	p-value of MWW tests
First Round	99.89 (13.46)	92.78 (17.53)	0.5796
Second Round	107 (15.07)	96.39 (15.65)	0.0439

Note: The difference in mean performance between males and females is not statistically significant in the first round but is statistically significant in the second round according to Mann-Whitney-Wilcoxon (MWW) tests. Standard Deviations are reported in parentheses.

C. Appendix to Chapter 4

C.1. Instructions

Original instructions are in German. Below is a translation of our instructions for the PI treatment in English. For the IP treatment, instructions for Stage 2 of Part A and Part B are switched.

Welcome! You are about to participate in an experiment funded by the Max Planck Institute of Economics. Please switch off your mobile and remain quiet. It is strictly forbidden to talk to the other participants. Whenever you have a question, please raise your hand and one of the experimenters will come to your aid.
You will receive 2.50 Euros for showing up on time. Besides this, you can earn more. The show-up fee and any additional amounts of money you may earn will be paid to you in cash at the end of the experiment. Payments are carried out privately, i.e., the others will not see your earnings.

During the experiment we shall speak of ECUs (Experimental Currency Unit) rather than Euros. The conversion rate between them is 1 ECUs = 0.3 Euro. This means that for each ECU you earn you will receive 0.3 Euro.

The experiment consists of two parts. The instructions for Part A follow on the next page. The instructions for Part B will be distributed after all participants have completed Part A. In each part we will explain to you how the earnings for that part are determined.

At the end of the experiment the computer will select either Part A or Part B for payment by a random draw. If Part A is selected, you will receive only the earnings you obtained in Part A; if Part B is selected, you will receive only the earnings you obtained in Part B. Therefore, since you do not know which part will be randomly selected for your final payment, please think carefully about each decision in each part.The earnings obtained in the selected part will be converted into Euros and paid privately in cash

.All instructions in both parts are identical for all participants and we read them aloud such that you can verify this.

Please note that Part A and Part B of the experiment are completely separated and you will never encounter the same person in both parts.

DETAILED INSTRUCTION FOR PART A.

1. What is the situation you are facing in Part A?

Part A will consist of 2 stages which will be conducted as follows: In Stage 1 all participants are asked to perform individually a task which involves counting zeros on the computer screen. The task will be explained in detail below and it is identical for each participant. In Stage 2 you will be randomly paired with another participant in this room to form a pair. You will not be told who you are paired with either during or after the experiment. A variable amount of ECUs will be credited to an account which is assigned to each pair. This amount is dependent on the performance of the two participants in the pair in Stage 1. Each member of the pair will know his own performance and that of the other participant in the pair.

In treatment PI only. The ECUs credited to each pair's account will be distributed to that pair. However, a given member of the pair cannot be guaranteed any specific amount of ECUs, since the final allocation of the ECUs between the two participants in the pair will depend only on the decision of one of the two participants in the pair, chosen randomly to make the distribution. The details and the manner of this distribution will be provided after Stage 1 is explained below.

In treatment IP only. The ECUs credited to each pair's account will be distributed to that pair. However, a given member of the pair cannot be guaranteed any specific amount of ECUs, since the final allocation of the ECUs between the two participants in the pair will depend only on your decision. The details and the manner of this distribution will be provided after Stage 1 is explained below.

2. Detailed information about Stage 1 of PART A.

In Stage 1 your task is to correctly count zeros in a series of tables in a 5 minutes. The figure below shows an example of the work screen you will encounter:

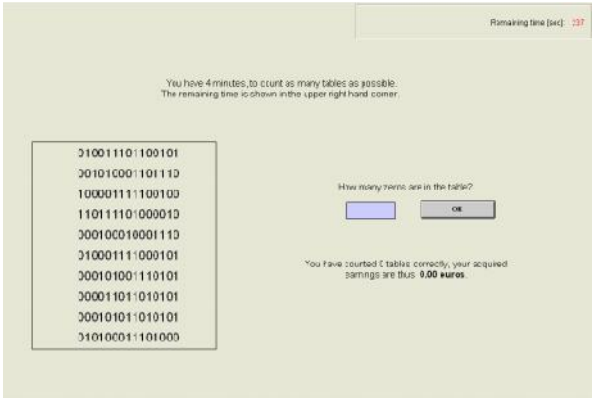


Figure C.1.: Screen shot of Real Effort Task

Each screen contains a table. For each table you have to enter the correct number of zeros into the box on the right side of the screen. After you have entered the number, click the OK-button. If you enter the correct result you get 1 ECUs. If your input was wrong, then you can try again two more times; If you do not submit any correct input in any of the three rounds you get 0 ECUs and proceed to the next table. Then a new table is generated, and so on for a total of 5 minutes.

At the end of Stage 1, your performance will be determined by the number of tables that you solved correctly within the 5 minutes. The more tables you solved correctly the higher your earnings. Before you start, each of you will face two practice rounds, which will not be relevant for the determination of you earnings. The practice rounds will be followed by the paying rounds with money prizes. In each round you will encounter a new randomly generated table.

Stage 1 will last 5 minutes and it will start after the practice rounds. Are there any questions?

2.1. Information at the end of Stage 1

At the end of this stage, we will determine your earnings depending on the number of tables that you solved correctly within the 5 minutes and you will be informed about your earning (i.e. the number of ECUs obtained). This is true for all participants.

3. Detailed information about Stage 2 of PART A

In Stage 2 you will be randomly paired with another participant. To each pair a certain amount of ECUs will be assigned, depending on the earnings

obtained by the two participants of the pair in Stage 1. So, in Stage 2 you will receive also information about the ECUs earned by the other person in your pair, (i.e. the number of ECUs obtained by each of you).

Example 1. Assume that in Stage 1 you earned 70 ECUs and the other participant in your pair earned 50 ECUs. Then, your pair will receive in total 120 ECUs, given by the sum of the two participants' earnings.

Example 2. Assume that in Stage 1 you earned 30 ECUs and the other participant in your pair earned 110 ECUs. Then, your pair will receive in total 140 ECUs, given by the sum of the two participants' earnings.

Once the ECUs are assigned to the pair, one of the two participants in the pair will be randomly chosen and he will have to decide how the total amount of ECUs assigned to the pair will be distributed between himself and the other person in the pair. So, the randomly chosen person has to indicate how many of the ECUs assigned to that pair he wishes to allocate to his counterpart in the pair. He can choose an amount between 0 and the total amount of ECUs assigned to the pair.

3.1. What is the decision you have to take in Stage 2?

All the participants in Stage 2 will have to make a decision as if they were randomly chosen to distribute the ECUs within their pair. After all participants have made their decision, the computer will randomly select one participant in each pair and his choice will be used to determine the ECUs distribution within the pair. Each participant has to make his decision without knowing the decision made by the other participants.

3.2. How your earnings in PART A will be determined?

Depending on whether you will be randomly selected or not, we have two cases:

Case 1. You ARE randomly selected.

Your choice about how to distribute the ECUs is relevant for your earnings. This happens with ½ probability. In this case your choice about how to allocate the ECUs between you and your counterpart will become effective.

Case 2. You ARE NOT randomly selected.

Your choice about how to distribute the ECUs is NOT relevant for your earnings. This happens with ½ probability. In this case, your earnings for this part will be determined by the choice of another participant. If this is the case, please note that this participant is not the one to whom your choice in the first case would apply.

Example. Imagine two pairs: one formed by A and B; the other formed by C and D. According to our protocol, all four participants take a decision on how to allocate the ECUs within a pair in Stage 2. Consider now Participant

A.

- In case 1, his earnings in part A just depend on his choice. Moreover, his decision also affects the earning of another participant, in our case B.

- In case 2, his earnings are determined by the choice of another participant. In this case, we impose that this participant must be different than B, so he will be, C or D.

3.3.Earnings from the experiment

You will know whether your choice is relevant for your earnings or not at the end of the experiment (i.e. after Part B ends). Remember that at the end of the experiment either Part A or Part B will be selected by a random draw and you will receive the earnings you obtained in the randomly selected part. Therefore, since you do not know which part will be relevant for your final payment, please think carefully about each decision in each part.

DETAILED INSTRUCTION FOR PART B.

1. What is the situation you are facing in Part B?

In Part B of the experiment you will be matched with two different participants. These two participants form a pair. The decision making in this part does not involve the participant you were matched with in Part A. You will not be told who you are matched with either during or after the experiment. In Part B you will be asked to make a decision about how to distribute a variable amount of ECUs assigned to this pair of participants. You will then receive information about the earnings of the participants in the pair (i.e. the number of ECUs earned in stage 1 of Part A).

Example 1. Assume that in Stage 1 of Part A the first participant in the pair earned 130 ECUs and the second earned 10 ECUs. Then, the total amount of ECUs credited to this pair is 140.

Example 2. Assume that in Stage 1 of Part A the first participant earned 50 ECUs and the second participant earned 50 ECUs. Then, the total amount of ECUs credited to the pair is 110.

You will be asked to indicate how many, if any, of the total amount of ECUs you wish to allocate to each participant in the pair. You can choose an amount between 0 and the total amount of ECUs assigned to the pair. This decision is completely up to you and is confidential. Just make sure that the sum of ECUs allocated to the two participants in the pair equals the total joint earnings shown. If part B is selected to be relevant for your

experimental earnings, you will earn the amount of ECUs, you obtained in Stage 1 of Part A.

2. What is the decision you have to take in Part B?

All the participants in Part B will have to make a decision as if they were chosen to distribute the ECUs for a pair of participants. Each participant has to make his decision without knowing the decision made by the other participants. The participant making the decision earns the amount of ECUs which derive from his earnings in Stage 1 of Part A. After each participant has made his decision, the computer randomly selects a participant, so his choice about how to distribute the ECUs in the pair becomes relevant for the members of a pair.

2.1. How will your earnings in PART B be determined?

Depending on whether you will be randomly selected or not, we have two cases:

Case 1. You ARE randomly selected.

Your choice about how to distribute the sum of ECUs for another pair is relevant for the earnings of the pair. This happens with 1/3 probability. In this case, your choice will become effective for the two participants in the pair and you will earn the amount of ECUs based on the earnings you earned in Stage 1 of Part A (counting zeros).

Case 2. You ARE NOT randomly selected.

Your choice about how to distribute the sum of ECUs for another pair is NOT relevant for the earnings of that pair. This happens with 2/3 probability. In this case, your earnings for Part B will be determined by the choice of another participant. If this is the case, please note that this participant is none of the two participants to whom your choice in the first case could have applied.

Example: Imagine a situation with 6 participants A, E, F, G, H and I. According to our protocol, all participants take a decision on how to allocate the ECUs within a pair in Part B. Consider now participant A and assume he is matched to participant E and F (which form a pair), while G is matched with participants H and I (which form another pair).

- In case 1, his earnings in part B just depend on his earnings in Stage 1 of Part A. However, his decision affects the earnings of a pair of participants, E and F.
- In case 2, his earnings are determined by the choice of another participant. In this case, we impose that this participant must be different

than E and F, so he will be, for H or I.

2.2 Earnings from the experiment

You will know whether your choice is relevant for your earnings or not at the end of the experiment (i.e. after this part ends). You will know whether your choice is relevant for the pair or not at the end of the experiment. Remember that at the end of the experiment either Part A or Part B will be selected by a random draw and you will receive the earnings you obtained in the randomly selected part. Therefore, since you do not know which part will be relevant for your final payment, please think carefully about each decision in each part.

C.2. Vignette study

We present the results of our vignette study conducted at the end of the experiment which follows Krupka and Weber (2013).

All participants were confronted with an allocation situation similar to the one encountered in the experimental tasks: a total amount of 100 ECUs had to be allocated between player 1 and 2 who contributed 40 and 60 ECUs, respectively. In our vignette study, we asked participants to evaluate the social appropriateness of 11 potential allocations obtained by starting from an allocation of 0 to Player 1 and of 100 to Player 2 and increasing (decreasing) in steps of 10.

For each allocation individuals had to guess the answer chosen by the majority of the participants on a 4-point scale with the following values: "Socially very unacceptable," "Socially quite unacceptable," "Socially quite acceptable," "Socially very acceptable." The aim of the task is therefore to guess the prevailing social norm in the reference population. Following Krupka and Weber (2013), we converted participants' social norm ratings into numerical scores. A rating of "Socially Very Unacceptable" receives a score of -1, "Socially Quite Unacceptable" a score of -1/3, "Socially Quite Acceptable" a score of + 1/3, and "Socially Very Acceptable" a score of + 1.[1]

Table C.1 presents participants' social appropriateness ratings across all treatments: each row corresponds to one possible allocation of resources.

[1] As Krupka and Weber (2013) discuss, this particular scoring is intuitive (the least and most appropriate possible ratings receive scores of - 1 and + 1$, respectively) and simple (possible ratings are evenly spaced over the - 1 to + 1 interval).

As shown in Table , which contains data from the four treatments, most of the participants think that extreme allocations are perceived as socially unacceptable. The second column reports the mean and standard deviation of the social appropriateness ratings (ranging from complete agreement on "very socially inappropriate" (*-1.0*) to complete agreement on "very socially appropriate" (*1.0)*, and then the full distribution of responses.The general pattern of social appropriateness ratings shows that the vast majority of participants deem the allocation exactly matches proportionality of inputs and rewards (i.e. 40—60) and the equal split allocation (i.e. 50—50) as socially acceptable.

Table C.1.: Social norms vignette responses

Allocation	Mean Rating	-1	-1/3	1/3	1
0—100	-0.948	**95.60**	2.20	1.10	1.10
	(0.268)				
10—90	-0.878	**85.00**	12.90	1.10	1.10
	(0.327)				
20—80	-0.578	42.20	**53.30**	3.30	1.10
	(0.404)				
30—70	-0.148	6.70	**60.00**	32.20	1.10
	(0.399)				
40—60	0.630	0.60	5.60	42.80	**51.10**
	(0.418)				
50—50	0.726	0.60	7.20	25.00	**67.20**
	(0.433)				
60—40	-0.093	13.30	**42.80**	38.30	5.60
	(0.0521)				
70—30	-0.515	42.20	**46.10**	8.30	3.30
	(0.502)				
80—20	0.752	**69.40**	26.70	1.10	2.80
	(0.435)				
90—10	-0.881	**90.00**	5.60	1.10	3.30
	(0.406)				
100—0	-0.907	**95.00**	0.00	1.10	3.90
	(0.409)				

Note: Standard deviations are in parentheses. The first number in the first column refers to what Player 1 (who contributed 40 in the real effort task) receives and the second number reers to what Player 2(who contributed 60) receives. The allocation 40-60 reflects strict adherence to the proportionality principle. Columns 3 to 5 show the proportion of participants who rated the allocation as 'Socially Very Unacceptable' (- -); 'Socially Quite Unacceptable' (-); 'Socially Quite Acceptable' (+); 'Socially Very Acceptable' (++) The figures in bold font identifies the assessment that participants think will be given by the majority.

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Erklärung gemäß § 4 Abs. 1 Pkt. 3 (PromO)

Hiermit erkläre ich,

1. dass mir die geltende Promotionsordnung bekannt ist;
2. dass ich die Dissertation selbst angefertigt, keine Textabschnitte eines Dritten oder eigener Prüfungsarbeiten ohne Kennzeichnung übernommen und alle von mir benutzten Hilfsmittel, persönliche Mitteilungen und Quellen in meiner Arbeit angegeben habe;
3. dass ich bei der Auswahl und Auswertung des Materials sowie bei der Herstellung des Manuskripts keine unzulässige Hilfe in Anspruch genommen habe;
4. dass ich nicht die Hilfe eines Promotionsberaters in Anspruch genommen habe und dass Dritte weder unmittelbar noch mittelbar geldwerte Leistungen von mir für Arbeiten erhalten haben, die im Zusammenhang mit dem Inhalt der vorgelegten Dissertation stehen;
5. dass ich die Dissertation noch nicht als Prüfungsarbeit für eine staatliche oder andere wissenschaftliche Prüfung eingereicht habe;
6. dass ich nicht die gleiche, eine in wesentlichen Teilen ähnliche oder eine andere Abhandlung bei einer anderen Hochschule bzw. anderen Fakultät als Dissertation eingereicht habe.

Jena, den 16.10.2015 .....

(Marian Panganiban)

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Liste der Veröffentlichungen, Arbeitspapiere und wissenschaftlichen Vorträge

Wissenschaftliche Veröffentlichung

J. Capuno and M. Panganiban, “"The ties that do not bind: Party affiliation and local service delivery in the Philippines," Philippine Political Science Journal 33(1):63-80.

Arbeitspapiere

M. Panganiban, “To friends everything, to strangers the law? An experiment on group identity and contract enforcement,” Jena Economic Research Papers #2015- 015

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